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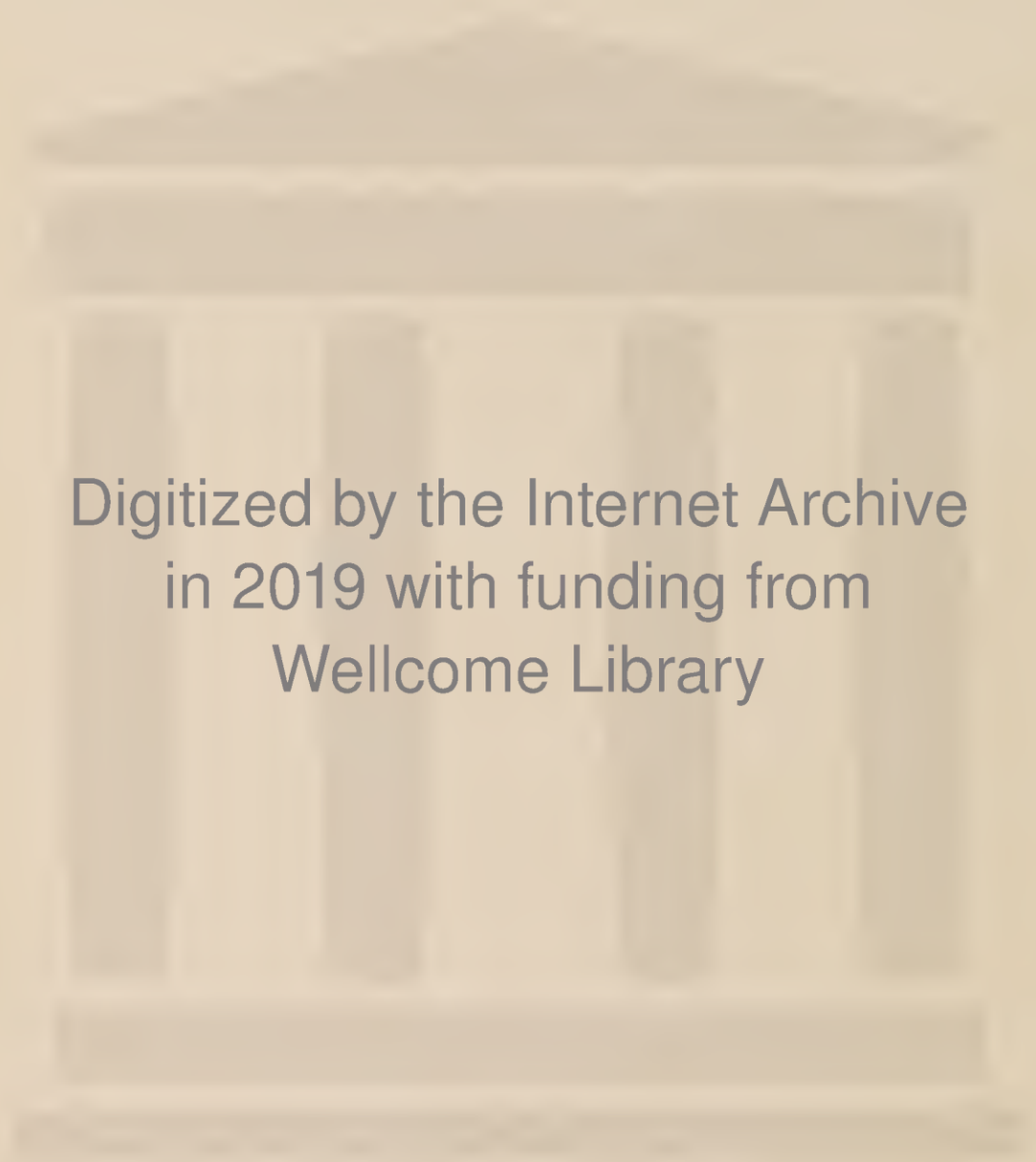
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A

JOURNAL

OF

NATURAL PHILOSOPHY,

CHEMISTRY,

AND

THE ARTS.

VOL. XII.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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1805

PREFACE.

THE Authors of Original Papers in the present Volume, are, Davies Giddy, Esq. M. P.; Mr. O. Gregory; Mr. William Close; Mr. John Dalton; W. N.; Mr. Charles Young; Mr. J. Stodart; Count Rumford, V. P R. S.; Mr. G. I. Singer; J. P.; T. S. Traill, M. D.; G. Cumberland, Esq.; Mr. S. Clegg; Mr. Dalton; T. I. B.; T. Plowman, Esq.; F. A.; N. D. Starck, R. N.; John Gough, Esq.; A Correspondent; An Enquirer; Mr. Arthur Woolf; Mr. Elizur Wright; Thomas Northmore, Esq.; Dr. Beddoes; Mr. Bancks.

Of Foreign Works, Professor Proust; J. H. Hassenfratz; G. A. Lampadius; Mr. Prony; Beaunier; Gallois; J. B. Richter; Mr. Ritter; Citizen Bernoulli; Vauquelin; C. A. Prieur; Guyton; Morveau; Chaptal; Dr. Valli; M. Vaucher; M. Hauffman; Biot; Thenard; Descotils; C. L. Cadet; Professor Pfaff; Tromsdorf; M. Achard; M. Steinacher; M. Giobert; Humboldt; M. Lagoux de Flaix; M. Dodun.

And of English Memoirs abridged or extracted, Humphrey Davy, Esq. F. R. S.; Mr. Rose; Benj. Smith Barton, M. D.; Mr. John Heckewelder; J. C. Curwen, Esq. M. P.; Mr. William Bartram; Thomas Andrew Knight, Esq.; Rev. D. Pape; William Herschel, L. L. D. F. R. S.; Charles Hatchett, Esq. F. R. S.; T. C. Hope, M. D. F. R. S. Ed.

Of

Of the Engravings, the Subjects are, 1. Apparatus for raising Water, by Mr. Close. 2. Furnace for bending Wood. 3, 4. Blowing Engine by the Fall of Water at Poullaouen. 5, 6. Apparatus for analysing Vegetable Soils, by H. Davy, Esq. 7. Diagrams to explain the Doctrine of Heat, by Count Rumford, V. P. R. S. 8. Magnified Representation of Insects found on Corn, by G. Cumberland, Esq. 9. An improved Steam Engine, by Mr. Clegg. 10. Simple Register Thermometers. 11. The Framing employed to raise the Roof of Clapham Church. 12. An improved Sheep Fold, by T. Plowman, Esq. 13. Applicative Compass, by Capt. Starck, R. N. 14. Section of a Drain, by J. C. Curwen, Esq. 15. Diagrams to illustrate a Series of Propositions respecting a Division of the Circle, by Mr. John Gough. 16. Schemes exhibiting the Positions of the new Planet Juno, by Dr. Herschel. 17. Mr. Pape's Improvement of Rye Harbour. 18. A new Air Pump, by Mr. Wright of America. 19. New Valve and Parts of a condensing Syringe for the Gases, by Mr. Cuthbertson, with which Mr. Northmore's Experiments were made. 20. A very simple and improved Graphometer, by Mr. Bancks.

Soho Square, London, January 1, 1806.

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ADVERTISEMENT.

THE Patrons and Friends of the Philoſophical Journal are reſpectfully informed, that this work will in future conſiſt of ſix ſheets of matter inſtead of five in each number, together with a ſupplementary number to each volume; and that the plates will be ſo managed as to contain a larger number of ſubjects executed in the beſt ſtile. Theſe improvements, though attended with additional expence, are ſuch as the Editor has thought it his duty to adopt; in order that the very extenſive communications with which the Journal has been honored, might not prevent him from giving all the foreign diſcoveries and ſuch other general intelligence as the nature of the plan demands. It will eaſily be ſeen that the additional copy will by this means amount to one half more than the former quantity; and it is unneceſſary to point out the great advantages which muſt reſult from ſuch an addition. The Editor takes this occaſion to repeat his acknowledgments for the encouragement which has been given to his exertions, particularly within the laſt twelve months, in which the ſale has nearly doubled. The quantity of original matter continues to increaſe, and amounts to more than half of the whole work. Great part of the remainder conſiſts in foreign articles never before publiſhed in this country, together with ſome extracts and abridgments from our beſt academical tranſactions. The whole publication may therefore be conſidered as original, ſince it is never made up by extracts from the periodical works of this country, which, on the contrary, very frequently copy from its contents.*

* It has been neceſſary to reprint a conſiderable part of the former volumes in order to make complete ſets, which may now be had, or any ſingle numbers or volumes, from the commencement.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

SEPTEMBER, 1805.

ARTICLE I.

Letter from DAVIES GIDDY, *Esq. M. P. describing a singular Fact of the invisible Emission of Steam and Smoke together from the Chimney of a Furnace; though either of them, if separately emitted, is visible as usual.*

To Mr. NICHOLSON.

SIR, *Clifton, August 6*, 1805.

Peculiar facts observed in working one of Trevithick's steam engines.

TRAVELLING, and a variety of occupations, have hitherto prevented me from sending you an account of the circumstances observed by myself and others, during the working of an engine on Mr. Trevithick's construction, at Merthyn Tidwell in South Wales, and which I had the pleasure of relating to you, some time since, in Soho Square. I now transmit a statement of the facts, avoiding all comments or attempts at explanation.

The steam engine was adapted to move a carriage, and every part made light and simple.

Mr. Trevithick having adapted his steam engine to the purpose of moving waggons, contrived every accessary part as light as he possibly could, and as little inconvenient to persons who might assist, or witness an experiment. The flue for conveying off the smoke, and affording a draft, was made

of rolled iron; and the ſteam, which wholly eſcapes from theſe machines uncondenſed, was conducted into the ſame tube, about a foot above its inſertion into the boiler; therefore many feet from the fire, and beyond the regiſter. When the engine began to move, it was ſoon remarked that neither ſteam nor ſmoke were ſeen to iſſue from the flue: and when freſh coal was added, nothing more than a faint white cloud became apparent, and that only for a ſhort time; nor were drops or miſt viſible any where. It was propoſed, that the regiſter ſhould be ſlowly cloſed; and as this was done, a condenſation of ſteam manifeſted itſelf at a ſmall diſtance from the chimney, and finally appeared in the ſame quantity, as if it had proceeded immediately from the boiler. The experiment was then reverſed. The ſteam was gradually confined to the boiler; when ſmoke became more and more viſible, till it equalled in quantity and appearance that commonly produced by a ſimilar fire: and theſe trials were alternated a great number of times, with unvarying ſucceſs. Laſtly, it became a matter of ſpeculation, whether or in what degree the draft was affected by the admiſſion of ſteam into the flue. To aſcertain this, every one preſent looked as attentively as poſſible into the fire-place; while the engine moved at the rate of a few ſtrokes in a minute; and all agreed in declaring, that the fire brightened each time the ſteam obtained admiſſion into the chimney, as the engine made its ſtroke.

The ſteam was (after work) thrown into the chimney many feet from the fire.

Neither ſmoke nor ſteam were viſible.

When the ſmoke was ſhut off the ſteam was viſible, and when the ſteam was ſhut off the ſmoke was viſible.

The draft up the chimney was increaſed by the admiſſion of the ſteam.

I am, Sir,

Your very faithful humble ſervant,

DAVIES GIDDY.

II.

On a Meteoric Stone that fell in the Neighbourhood of Sigena, in Arragon, in 1773, *by Profeſſor* PROUST*.

THE author begins his paper by ſome previous hiſtorical facts. No one now queſtions, that ſtones have fallen from the atmoſphere in different parts of the world. The ancients

Stones have fallen from the atmoſphere in various ages,

* Abridged from the Journal de Phyſique, for March 1805.

mention

mention it as having occurred at various times, and later ages have recorded the time and authenticated circumſtances of ſeveral ſuch incidents. In our own days, ſtones or mineral bodies termed meteoric, have been collected in the Eaſt Indies, America, Scotland, England, France, Italy, Hungary, and laſtly in Spain: and that nothing may be wanting in future to convince thoſe who refuſe their aſſent to the united teſtimony of all ages and all countries, nature appears to have purpoſely ordered a repetition of this ſurpriſing phenomenon: no longer ago than the 26th of April, 1803, a ſhower of theſe ſtones covered a ſpace of ground two miles long and above a mile broad, near l'Aigle in Normandy. The French Inſtitute immediately nominated a commiſſioner, to examine into the fact on the ſpot, to take the depoſitions of witneſſes, compare them with the circumſtances, and bring ſome of the ſtones to Paris.

and in all parts of the world.

Shower of ſtones in 1803.

As the firſt thing to be done with a new mineralogical ſubſtance, is, to analyſe it, the Preſident of the Royal Society of London, and ſeveral other gentlemen who had ſuch ſtones in their collections, put them into the hands of Mr. Howard, a Member of the Society, that he might ſubject them to chemical examination. He found to his great ſurpriſe, that all theſe ſtones, from the remoteſt quarters of the globe, contained the ſame principles, differing only in proportion; and, what was ſtill more ſtriking, that they all contained iron combined with nickel, a compound to be met with among none of the minerals in any part of the globe with which we are acquainted. Vauquelin has ſince confirmed by repeated experiments, the accuracy of Mr. Howard's obſervations. All men of ſcience have hence been led to conclude, that theſe ſtones muſt have a common origin; but whence they originate is the queſtion. Do they belong to that earth on which they fall? are they formed in the atmoſphere itſelf? or have they been projected from lunar volcanoes? On theſe points men's ſentiments are divided; and the arguments have been collected by Dr. Izarn, in his *Lithologie Atmoſphérique*.

Analyſis of many of theſe bodies by Mr. Howard,

who found them all ſimilar in their compoſition.

They have therefore a common origin,

but whence?

One of theſe ſtones has been in the royal collection at Madrid ever ſince 1773. This the miniſter has allowed Mr. P. to analyſe, leaving the principal part of it ſtill in the collection for the ſatisfaction of the curious. The following letter was ſent with it to Don Manuel de Roda, Miniſter of State, by the captain-general of Saragoſſa.

One in the collection at Madrid.

Account of its fall.

"In November laſt an extraordinary occurrence, ſaid to have happened on the Seventeenth of that Month in a ploughed field at Sena, a village in the diſtrict of Sigena, was the ſubject of converſation in this city.

"The ſky being perfectly ſerene, three reports reſembling thoſe of cannon were heard, and followed by the fall of a ſtone weighing nine pounds and one ounce, at a little diſtance from two labouring men. One of them went up to it, but the ſtrong ſmell it emitted ſtopped him a moment.

"Recovering from his ſurpriſe he went nearer, raiſed it up with his ſpade, and waited till it was ſufficiently cold for him to carry it to the village, where he delivered it to the prieſt.

"From inquiries made immediately afterwards on the ſpot, and among the people in the neighbourhood, it appears, that the noiſe in the air and fall of the ſtone were not accompanied with any ſtorm, or with lightning."

Another meteoric ſhower of ſtones that fell at Roa in Spain, in 1438,

To bring into one view all that is yet known of ſtones falling in Spain, the author ſubjoins a letter of the Bachelor Cibdadreal, on thoſe that fell in the village of Roa, near Burgos, in 1438.

in the view of the King of Spain.

"While the King Don John and his court were hawking near the village of Roa, the ſun was concealed behind white clouds, and bodies reſembling gray and blackiſh ſtones were ſeen to fall from the air, of ſuch bulk as to occaſion the greateſt ſurpriſe.

The ground was covered with them.

"After this phenomenon had continued for an hour the ſun re-appeared, and the falconers immediately rode to the place, which was not above a mile diſtant. They brought back information to the king, that the ground was ſo completely covered with ſtones of all ſizes as not to be viſible.

"The king would have gone thither, but his courtiers prevented him, obſerving, that a place choſen by Heaven for the theatre of its operations might not be free from danger, and that he had better ſend ſome of his attendants. Gomez Bravo, the captain of his guards, undertook the office. He brought four of the ſtones to Roa, whither the king had already retired. They were of conſiderable ſize: ſome were round, and as large as a mortar, others like pillows and *half-fun que* meaſures (ſuch as contain about 45lbs weight of corn,) but what was moſt aſtoniſhing, was their exceſſive lightneſs, ſince

These stones were very large,

extremely light,

the largeſt did not weigh half a pound. They were of ſuch a tender texture, that they reſembled the ſoam of the ſea condenſed more than any thing. You might ſtrike on them with your hand without fear of bruiſe, or pain, or the ſlighteſt mark. The king has ordered ſome to be ſent you, &c."

and of a tender texture.

It ſeems from this deſcription, that theſe ſtones muſt have been very different from thoſe of the preſent day.

The ſtone of Sigena, when delivered to Mr. Prouſt, weighed ſix pounds ten ounces. With it was a piece of three or four ounces, the only one remaining of thoſe that had been broken from it by curious perſons. It was interſperſed with ſpots of ruſt, both externally and internally, owing probably to its having been immerſed in water to try the effect of that fluid on it. From theſe however, ſome inſtructive inferences may be drawn reſpecting the native place of theſe ſtones.

The ſtone of Sigena deſcribed.

Interſperſed with ruſty ſpots.

Its ſhape is an irregular oval, ſeven or eight inches long, four or five broad, and four in its greateſt thickneſs. One ſide is flattiſh, a little depreſſed in the middle, and very round on the edges: the other is an obtuſe triedral pyramid with unequal ſides, greatly rounded at the ſummit and on the edges *. It appears to have had the black vitreous cruſt common to ſtones of this kind, though from its fragility the greater part has fallen off in paſſing through many hands and receiving occaſional blows, ſo that none remains except in the hollow of the baſe, and a little on the faces of the pyramid.

Its figure.

It had a vitreous cruſt.

On examining this cruſt it is eaſy to ſee, that it muſt have been the effect of heat ſubſequent to the formation of the ſtone, and unqueſtionably very powerful though momentary; ſince the metallic and ſulphureous particles immediately beneath the cruſt had not time to change colour, or even loſe their luſtre.

This muſt have been produced by violent momentary heat ſince its formation.

It has all the poroſity of an aggregated maſs of ſandy particles without any cement, ſo that the breath will eaſily paſs through a piece held between the teeth. It will not ſtrike fire with ſteel, and the ſame may be ſaid of the pyrites it contains.

It is porous, not very hard,

Its colour is a uniform bluiſh-gray, like that of a black ſubſtance enlightened by a white: it is the hue of an earthy compound tinged by iron oxided at a *minimum*.

of a bluiſh gray.

* Does this deſcription agree with what is ſaid above, that ſeveral pieces have been broken from it? Apparently above a quarter of the ſtone, on comparing its original and preſent weight. J. C.

The

It is a ſandy maſs interſperſed with metallic and ſulphureous particles.

The ſtone itſelf is a ſandy maſs, formed of rounded oval grains, the largeſt of which are ſcarcely bigger than hemp-ſeed, among which are interſperſed metallic and ſulphurious particles with all their primitive luſtre, and particularly with that light tint of kupfernickel obſerved in the other ſtones.

Its granules are cryſtalline.

On examining the earthy grains by the microſcope, we perceive, that, far from having been faſhioned by the movement of water, they are globules rough with cryſtalline or reflecting points, ſo that they can by no means be confounded with ſand.

Heat deepened its colour, and oxided the metal.

A piece of about two inches being expoſed to a red heat in a crucible for half a quarter of an hour was much changed: the ſandy globules became of a darker gray, and the metallic particles, diveſted of their luſtre, were viſibly oxided.

Greater heat fuſed it.

About two ounces were heated for half an hour in a forge fire, which converted the ſtone into a ſemivitreous maſs, blackiſh, and ſlightly porous. It did not appear to have effervеſced much previous to fuſion, and was interſperſed with globules of iron, which had not time to deſcend, though upwards of a hundred grains of a regulus were collected at the bottom.

It contained much magnetic iron,

The iron attractable by the magnet was not uniformly mixed in the ſtone, as from ſome parts 22 in the 100 were extracted, from others not more than 17.

combined with nickel.

This iron was combined with nickel in the proportion of about 3 per cent. No nickel was diſcoverable in any other part of the ſtone.

After this alloy was ſeparated by the magnet, the remainder of the ſtone was found by analyſis to conſiſt of,

Conſtituent parts of the remainder.

Iron ſulphurated at a *minimum* -	12
Black oxide of iron - - -	5
Silex - - - -	66
Magneſia - - - -	20
Lime and magneſia in quantities too ſmall to be appreciated - -	
	103

New hypotheſis. Their origin probably in the polar regions,

On conſidering the rapid alteration of theſe ſtones by moiſture, for a fragment kept twelve hours under water was taken out covered with ſpots of ruſt, which diſtinguiſhed the grains of alloy from the ſulphureous particles with which they were before confounded;—it is obvious, according to the

author

author, that they cannot ſubſiſt in any of the habitable parts of the globe. But from the eternal cold of the polar regions, where water remains for ever a ſolid maſs, and iron cannot ruſt, he thinks we may reaſonably look to theſe regions as the native place of ſuch bodies. In this he inſiſts there is nothing impoſſible, or even improbable. And why ſhould thoſe meteors, he demands, of which we know neither the origin, the combuſtibles that afford them aliment, the impulſe by which they are moved, nor the nature of the lines they deſcribe in their courſe, be leſs capable of tearing them from ſome part of the globe, than of forming them, contrary to all phyſical probability, from elements which the atmoſphere can neither create nor hold in ſolution?

whence they are conveyed to other parts by meteors.

III.

Farther Remarks on Mechanic Power, in Reply to Mr. J. C. Hornblower. In a Letter from Mr. O. GREGORY, *Royal Mil. Academy.*

To Mr. NICHOLSON.

SIR,

I AM ſorry to be under the neceſſity of troubling you with a few obſervations for inſertion in your Journal, in conſequence of being called upon by Mr. Hornblower, as though it were to defend ſome newfangled doctrine, when the poſitions in my former letter, which that gentleman thinks proper to cenſure, are in perfect conformity with the principles aſſumed or demonſtrated by every correct writer on mechanical philoſophy ſince it has been placed upon its proper baſis in the *Principiæ* of Newton. The ſubject I am now invited to diſcuſs, has ſo frequently been exhibited in the cleareſt light by various authors, both in England and on the Continent, that I ſhould not think myſelf juſtified in occupying many of your pages by an elaborate diſſertation; out of regard, however, to ſo reſpectable a correſpondent as Mr. H. I cannot help entering a little into the diſcuſſion, though I am, I confeſs, quite unable to aſcertain whether his laſt letter is meant to oppoſe my former remarks, and thoſe of Profeſſor Robiſon with ſerious arguments, or is merely intended as a *Jeu d'eſprit.*

Prefatory remarks.

It

It will not, I hope, be expected that I ſhould point out in what inſtances Mr. H's remarks appear to me completely irrelative to the ſubject in hand; or thoſe in which he ſeems to have miſunderſtood the arguments of the late learned Profeſſor: ſuch a procedure would only lead into farther diſcuſſion, while I feel ſolicitous to avoid it, from a conſciouſneſs that it would be very unintereſting to moſt of your readers. I ſhall ſtrive to confine myſelf, therefore, to ſuch of Mr. H's enquiries as bear upon the point in diſpute, and for the ſake of condenſing my labour, ſhall begin with that in his poſtſcript.

Are animal exertion and mechanic power identical?

"Firſt then, I will endeavour to "ſet Mr. H. right as to the identity of *animal exertion* and *mechanic power*:" and to this end it will be requiſite to anſwer the queſtion,—what *is* mechanic power? excluding, for the preſent, that acceptation of the term in which it is underſtood to denote one of the ſix ſimple machines. Now, it is pretty obvious, that the terms power, force, &c. when uſed in mechanical ſcience are purely metaphorical; for, as Profeſſor Dugald Steward remarks, (*Elements of the Philoſophy of the Human Mind*, p. 202.) "All the languages which have hitherto exiſted in the world, have derived their origin from *popular uſe*; and their application to philoſophical purpoſes was altogether out of the view of thoſe men who firſt employed them." Language commenced amongſt ſimple men, who had little, if any acquaintance with what is now called ſcience: and in the gradual progreſs of moſt nations, from the ſavage to the ſhepherd ſtate, thence to the agricultural, and farther to the commercial ſtate, it would be very long before they would think of attaching any other meaning to the terms in the different languages, correſponding to power, force, action, reſiſtance, repulſion, &c. than thoſe which were manifeſtly referable to the different kinds of human, or of animal exertion: in ſubſequent times when ſcientific men began to claſſify, arrange, and ſyſtematize the phenomena which they obſerved in the congreſs, motion, and mutual operation of bodies, they found it much eaſier to denote the circumſtances they would deſcribe or treat of, by a figurative application of old terms, to which ſome analogous notions would neceſſarily be attached by every perſon, than to invent new ones, which would be attended by no ideas independant of an arbitrary definition. Nor, in this application, was there any danger of important

Riſe and application of the terms, force, power, &c.

error

error, for the things to which the terms were appropriated would exhibit ſuch ſpecific differences as would almoſt entirely preclude the chance of confounding one with another; and leave no room to fear, that when the terms were applied to inanimate beings, they would be concluded to exert ſtrength, or to poſſeſs power, as animals did; any more than we ſhould now fear being miſunderſtood when we ſpeak of the force of arguments, the attractions of benevolence, the faſcinations of beauty, or the repulſive tendency of envy. Thus, from contemplating the proceſs of this gradual refinement, (a refinement produced not by barren ſpeculators, but by the neceſſary demands occaſioned by the progreſs of civilization,) we ſee that the words power and force, primarily uſed to denote animal energy, are now, by a natural extenſion grounded upon an obvious analogy, employed to expreſs *efficiency* in general. It will hence be eaſy to aſſign the proper philoſophical acceptation of theſe terms, when uſed in the ſcience of mechanics. *Force or power, in a mechanical ſenſe, is that, whatever it be which cauſes a change in the ſtate of a body, whether that ſtate be reſt or motion.* This definition does not require our entering into any metaphyſical diſquiſitions relative to the nature of cauſes, or the connection of cauſe and effect: that every event is brought about by ſome cauſe, that is, by ſome agency, or ſomething which precedes in the order of occurrence, is a truth which I think none will be diſpoſed to deny; but what is the agency, or where it actually reſides, we can ſeldom know, except perhaps in the caſe of our own voluntary actions. It is not then the buſineſs of the mechaniſt, ſtrictly ſpeaking, to enquire into the *modus operandi*; we learn from univerſal experience, that the muſcular energy of animals, the operation of gravity, electricity, impact, preſſure, &c. are ſources of motion, or of modifications of motion; and hence, without pretending to know any thing of the eſſence of either of theſe, we do not heſitate to call them mechanical forces; becauſe it is incontrovertible that bodies expoſed to the free action of either, are put into motion, or have the ſtate of their motion changed. Forces therefore, being only known to us by their effects, can only be meaſured by the effects they produce in like circumſtances, whether thoſe effects be creating, accelerating, retarding, deflecting, or preventing motions: and it is by comparing theſe effects,

Definition of the terms force and power.

Forces only known to us by their effects, thence meaſureable.

or

or by referring them to ſome common meaſure of ready appreciation, that mechanics is made one of the mathematical ſciences *.

Animal efforts are a *ſpecies* of mechanic power.

Theſe obſervations will enable us to ſet Mr. H. "right as to the identity of *animal exertion* and *mechanic power*." Animal efforts are juſtly conſidered, both by the Mathematician and the practical Engineer, as conſtituting *one ſpecies* of mechanic power; when theſe efforts give to bodies equal momenta, or give to equal bodies equal velocities, it is truly ſaid the animals exert equal forces; and we ſay that animal power is greater or leſs, as it is capable of imparting to bodies greater or leſs momenta, or as it is capable of ſtopping bodies moving with greater or leſs momenta: and the language of ſcientific men is analogous to this when they ſpeak of any forces whatever.

Reply to Mr. H's remarks on a former ſtatement.

It is now time to proceed to Mr. Hornblower's animadverſions upon the inſtance I adduced (and many others might be adduced) to ſhew that Mr. Smeaton's meaſure of mechanical power and effect is not univerſally applicable. I aſſerted,

* That Mr. Hornblower may not reſt upon the mere authority of any theoretic man, I beg to throw into this note an extract from the *Mechanique Philoſophique* of M. Prony, an Engineer, who unites with a profound acquaintance with the theory, an extenſive practice, and whoſe example in this reſpect I ſhould ſincerely rejoice to ſee more frequently imitated in this country.

"La nature de cette cauſe de mouvement, nommée *force* ou *puiſſance*; nous eſt tout-á-fait inconnue: l'homme appelle *force* la faculté organique qu'il a de ſe mouvoir, de s'arrêter, de produire ou de faire ceſſer le mouvement des corps qui l'environnent; et ſans ſavoir en quoi conſiſte cette faculté, il a ſuppoſé qu'il exiſtait quelque choſe de ſemblable dans les agens phyſiques qui ſont ou qu'il croit être, ſur le globe terreſtre et dans l'univers, les cauſes du mouvement de différens corps. Mais nous n'avons en mecanique, aucun beſoin de connaître la nature de la *force* ou *puiſſance* qui eſt repréſentée, meſurée et introduite, dans le calcul, uniquement par les *effets* qu'elle produit. Ces effets ſe réduiſent toujours à des vîteſſes que les puiſſances ou tendent à donner, ou ont effectivement données à de certaines maſſes." "Parmi les diverſes puiſſances que la nature nous offre, il en eſt une très remarquable dont il convient de prendre les effets pour terme de comparaiſon de ceux des autres puiſſances; c'eſt la *peſanteur terreſtre* a la ſurface de la terre," &c. p. 20.

that

that a horſe ſtanding ſtill and ſuſtaining a weight which hung by a cord over a fixed pulley, would, after a due interval of time, be completely fatigued, although neither the animal nor the weight moved, and that, of conſequence, there was a power expended, of which Mr. Smeaton's rule did not furniſh an adequate meaſure. Mr. H. as though he underſtood me to affirm, that fatigue was the *only* indication of mechanical power expended, inſtead of limiting it to animal efforts as the connection evidently required, exclaims, "it is really difficult to be grave on this occaſion:" p. 268. and argues in a kind of exulting ſtrain which ſavours a little, I am afraid, of the ſpirit alluded to in the French proverb, *Chanter le triomphe avant la victoire!* Let us, ſays this gentleman, have a "*poſt* inſtead of the horſe, and ſurely that will not tire, and what will be the conſequence then? why then there will be no power expended, and no effect produced." Mr. H. then, it would ſeem, has forgotten that the poſt is retained in *its* ſituation by a force which in this caſe oppoſes that of gravity acting upon the ſuſpended weight. The cord running over the pulley and ſuſtaining the weight, being faſtened to the poſt, would move it, were it not that the cohefive force of the earth in which the poſt is fixed, changes the ſtate into which the poſt would be brought by the action of gravity upon the weight, and is ſufficient to retain the whole at reſt. If the poſt were ſet in looſe ſand, or in a quagmire, the weight would draw it away, and then I ſuppoſe, even according to Mr. H's notion, there would be a power expended, and an effect produced. So likewiſe, in Mr. H's other example, of the hat hung upon the pin, the force of gravity is balanced by the cohefive force of the wood or other matter, into which the pin is fixed. But it would be egregious trifling to dwell much longer upon ſuch inſtances as theſe. Mr. H. conceives, if I have not completely miſunderſtood his meaning, that there is no "mechanical power" that is not "made up of a maſs of matter moving with a determinate velocity;" and as ſuch an opinion muſt either ariſe from neglecting to diſcriminate between cauſe and effect, or from a virtual denial of the whole doctrines of ſtatics, (in which powers are excited without any motion being produced,) I ſhall hope to be excuſed though I waſte no time on a refutation of any ſuch poſition.

Reply to Mr. H's remarks on a former ſtatement.

Indeed,

Indeed, much of Mr. H's reaſoning, not only with reſpect to the poſt and the hat-peg, but throughout his paper, ſeems to reſt upon a tacit admiſſion (not a direct avowal, it is true,) of the erroneous notion, that forces exerted by animated beings, and thoſe operating through the intervention of inanimate things, are totally diſtinct, and cannot be ſubſtituted the one for the other, or have a fair compariſon inſtituted between them. Whereas, on the contrary, not only the theory but the practice of mechanics, proceeds upon the principle, that thoſe forces are equal in degree, however different in their origin, or various in their mode of operation, which produce equal effects. Thus, for a familiar example, in the boring of a piece of ordnance; the borer may either be brought up to its proper poſition in the gun by the action of a man on the handles of a wheel, connected with the borer by rack and pinion work, or by the action of a weight attached to the farther end of a lever proceeding from the axle of the ſame wheel: and Mr. H. might as well deny the poſſibility of the work performed being the ſame in both theſe caſes, as deny that a weight is kept from falling by an equal force, when prevented either by an animal, or by a fixed inanimate object; or deny that there is an expenditure of mechanic power when a man counteracts the operation of gravity upon his arm, when extended horizontally. While ſpeaking of the propoſition which includes any ſuch denial, we may ſafely apply to it Mr. H's own language;—"A more erroneous propoſition was never introduced into the theory or practice of mechanics."

Familiar illuſtration.

Mr. Hornblower has taken the trouble of extracting ſeveral paſſages from the Article MACHINERY, *Sup. Ency. Britan.* and among them has taken that which exhibits Profeſſor Robiſon's meaſure of the exertion of a man, who walks at the rate of 60 feet per minute, and raiſes a weight of 30 pounds. The meaſure 57600, which this gentleman thinks enormouſly too large, is, in fact, too ſmall, in ſo far as it does not include that part of the exertion required by the man to move himſelf. It was this omiſſion of the learned Profeſſor that induced me to lay down the general ſtatement at p. 152 of your 43d Number, though I thought it might be deemed inviduous if I ſpecified my motives in that place. But when Mr. H. had commenced the labour of extracting, it would

Miſtake of Profeſſor Robiſon.

ſurely

ſurely have been but candid to produce a paſſage which ſtrikes with great force againſt the univerſality of Mr. Smeaton's meaſure, at the ſame time that it admits the utility of this meaſure to engineers in many caſes. This paſſage is as follows:—"When a weight of five pounds is employed to drag up a weight of three pounds, by means of a thread over a pulley, it deſcends with a motion uniformly accelerated, four feet in the firſt ſecond. Mr. Smeaton would call this an expenditure of a mechanical power 20. The weight three pounds is raiſed four feet. Mr. Smeaton would call this a mechanical effect 12. Therefore the effect produced is not adequate to the power expended. But the fact is, that the preſſure, ſtrain, or mechanical power, really exerted in this experiment, is neither five nor three pounds: the five pound weight would have fallen 16 feet, but it falls only four. A force has therefore acted on it ſufficient to make it deſcribe 12 feet in a ſecond, with a uniformly accelerated motion, for it has counteracted ſo much of its weight. The thread was ſtrained with a force equal to $3\frac{3}{4}$ pounds, or $\frac{3}{4}$ of 5 pounds. In like manner, the three pound weight would have fallen 16 feet; but it was raiſed four feet. Here was a change preciſely equal to the other. A force of $3\frac{3}{4}$ pounds acting on a maſs whoſe matter is only three, will in a ſecond, cauſe it to deſcribe 20 feet with a uniformly accelerated motion. Now 5×12 and 3×20, give the ſame product 60. And thus we ſee, that *the quantity of motion extinguiſhed or produced, and not the product of the weight and height, is the true unequivocal meaſure of mechanical power really expended, or the mechanical effect really produced; and that theſe two are always equal and oppoſite.* At the ſame time, Mr. Smeaton's theorem merits the attention of engineers; becauſe it generally meaſures the opportunities that we have for procuring the exertion of power. In *ſome* ſenſe, Mr. Smeaton may ſay, that the quantity of water multiplied by the height from which it deſcends in working our machines, is the meaſure of the power expended; becauſe we muſt raiſe this quantity to the dam again, in order to have the ſame uſe of it. It is expended, but not employed, for the water at leaving the wheel is ſtill able to do ſomething."

Robiſon on Smeaton's meaſure.

In oppoſition to all this, Mr. H. I ſuppoſe, would ſay that this is not a caſe in point, becauſe, "if the weight deſcends quickly

Meſſrs. Hornblower and Smeaton both concede the point in debate.

quickly it is ſenſibly compounded with another law, viz. the acceleration of gravity." Or, adopting other language of Mr. Smeaton, he might reſtrict his meaſures to "the height through which a body *ſlowly* and *equally* deſcended, or to which it was raiſed." But if, inſtead of the body's aſcending or deſcending ſlowly and equally, it moved rapidly and irregularly; or, if the motion was reciprocating, the velocity increaſing from quieſcence to a certain magnitude, and diminiſhing to quieſcence again; or, if we refer to the retarded riſe and accelerated fall of heavy ſtampers; in ſuch caſes if Smeaton's meaſure be applicable, I wiſh to ſee its manner of application explained; and if it be not univerſally applicable, a point which is, in reality conceded both by Mr. H. and Mr. Smeaton, there is then as to this head no ground of difference between us, and Mr. H's laſt letter becomes in a great meaſure a ſuperfluous labour; for, admitting the want of univerſality in the rule, is admitting all that I affirmed. Had not the meaſure been often very injudiciouſly exhibited as *univerſal*, a thing which Mr. Smeaton himſelf certainly never intended, I ſhould not have at all referred to it in my former paper.

In correct language *weight* and *heavineſs* ought to be diſtinguiſhed.

It may be deemed a ſlight deviation from the immediate object of this letter, but I truſt a juſtifiable one, if I briefly notice the ſurprize expreſſed by Mr. H. on account of Profeſſor Robiſon's diſtinguiſhing between weight and heavineſs. That the three terms *gravity*, *weight*, and *heavineſs*, admit of a palpable and obvious diſtinction, is, in my opinion, indubitable: and till this time, I imagined it was univerſally reckoned one great excellence of an accurate philoſophical diſquiſition, that it compriſed a careful diſcrimination of the various acceptations of theſe and other terms, which were commonly reputed ſynonimous. There may, undoubtedly, be occaſions in which a cautious ſelection from words of nearly ſimilar import may be diſpenſed with: but there are many more, particularly when handling philoſophical topics, where this careful choice cannot be ſafely neglected. And an attention to this point appears the more neceſſary, when it is recollected, that greater part of the controverſies which have been agitated by men of ſcience, have been rather verbal, than relative to things in themſelves. To contend for the uſe of many terms to expreſs one idea, inſtead of ſeeking for

for adequate ſeparate expreſſions to denote every idea the mind can form, is to ſacrifice preciſion and accuracy at the ſhrine of an ill-judged ſuperfluity. The common reſemblance between words eſteemed ſynonimous, does not comprehend the aggregate ſignification, but ſome iſulated particular attendant upon all, in ſome ſuch manner as may be traced in individuals of the ſame ſpecies: there is generally one, if not more qualities, on which a manifeſt diſtinction depends; and the determination of ſuch qualities is highly deſerving the notice, not only of the linguiſt, but of all who aim at philoſophical preciſion. I have not leiſure to look attentively over twenty-five cloſely printed quarto pages, in order to find how Profeſſor Robiſon diſtinguiſhes heavineſs from weight. But the labour is unneceſſary; for the diſtinction has often been made; and I will take the liberty of delineating it in the words of an author who is in no danger of having his ſentiments warped to ſquare with the tenets of any ſpeculative mechanical ſyſtem: I now advert to Dr. Truſler, who in his work on ſynonimous words ſpeaks thus:—

Truſler's remarks on heavineſs and weight.

"*Heavineſs*, *weight*.—In the figurative ſenſe the difference of theſe words is ſo extremely great, as needs no pointing out; in the literal indeed, they are often confounded: conſidered then in this laſt ſenſe, *heavineſs* is that quality in a body which we feel, and diſtinguiſh by itſelf: *weight* is the meaſure and degree of that quality, which we cannot aſcertain but by compariſon.—We ſay abſolutely, and in an undetermined ſenſe, that a thing is *heavy*; but relatively, and in a manner determined, that it is of ſuch a *weight*, for example, of two, three, or four pounds.—A thouſand circumſtances prove the *heavineſs* of the air; and the mercury determines its exact *weight*." Vol. I. p. 133.

My letter has attained a much greater magnitude than was at firſt intended, and I will now conclude it. The remarks I have been tempted to offer, are founded upon the moſt correct interpretation I could put upon Mr. Hornblower's language; and if I have any where miſunderſtood his meaning, I ſhall be pleaſed to ſee that miſunderſtanding candidly removed. I entertain great reſpect for that gentleman's talents as a practical engineer; though I cannot but think him completely wrong in moſt of thoſe remarks which have occaſioned this communication. I have replied to ſuch of his

ſtrictures

ſtrictures as bore any relation to myſelf, I believe, without acrimony: but I have a deeply rooted averſion to every thing that wears the garb of controverſy, and ardently hope the diſcuſſion on my part will be permitted to terminate here.

I am, Sir,

Your's, with much reſpect,

OLINTHUS GREGORY.

Royal Mil. Academy,
Woolwich, Aug. 9, 1805.

IV.

Deſcription and Effects of an Apparatus for raiſing Water by Means of Air condenſed in its Deſcent through an inverted Syphon. By Mr. WILLIAM CLOSE. *From the Inventor.*

To Mr. NICHOLSON.

SIR, *Dalton, July 27, 1805.*

Reference to the author's ſyphon engine.

IN one of my letters, ſome time ago, I briefly noticed an experiment I had made, to determine the practical value of the hydraulic machine, or inverted ſyphon, repreſented and deſcribed in the firſt volume of the preſent ſeries of your Journal *, obſerving, that, at ſome future period, I might probably tranſmit to you a more particular account. Having ſince repeated the experiment, I now ſend you a letter upon the ſubject, for I am of opinion that a machine operating upon the principle, when conſtructed in the manner herein deſcribed, will anſwer very well, in certain ſituations, to raiſe water for domeſtic purpoſes; and although it may not be competent to perform half as much work as a bucket engine by a forcing pump, yet it may be kept continually employed, and be ſubject to very little wear, as its operation will almoſt be performed without friction.

Deſcription of another apparatus;

The inverted ſyphon when applied to raiſe water in the manner deſcribed in this letter, has its higher orifice placed in a ſituation to receive both air and water at the ſame time. The air being conveyed by the velocity of the aqueous column

* See Philoſ. Journal, Vol. I. p. 30, Pl. IV.

to

to the loweſt part of the ſyphon, and collected in a veſſel, is employed as the medium for conveying preſſure to raiſe water in another part of the apparatus.

In May 1803, I determined to find by experiment, under what degree of preſſure it would be moſt advantageous to collect the condenſed air, and likewiſe the proportion then exiſting between the two fluids moving in the ſyphon. The apparatus conſtructed for this purpoſe, is repreſented in *Plate I. Fig.* 1. It required only a ſmall ſupply of water, but condenſed the air ſufficiently to be employed in the actual conſtruction of a machine upon the principle.

being an inverted ſyphon, which carries down air and condenſes it;

A round vertical pipe A B, half an inch in diameter, and 22 feet 5 inches in length, had its higher end placed in the ciſtern A, and its lower connected to a ſmall oblong veſſel C, which had an inverted glaſs bottle cemented upon a projecting cylinder on its upper ſide. From the other end of the veſſel aſcended another vertical pipe D E, half an inch in diameter, and 18 feet 3 inches in length, and terminated in a crook, 4 feet 2 inches below the higheſt part of the pipe A B.

The whole apparatus being filled with water, the ciſtern having a conſtant ſupply ſufficient to keep the ſurface of the fluid juſt above the orifice of the pipe A B, when the orifice of emiſſion at E was opened, the water flowing through A B, carried bubbles of air into the veſſel C, which aſcending, diſplaced the water in the bottle, and afterwards that contained in the veſſel C, above the lower ends of the pipes A B and D E. At the firſt efflux, and after the deſcent of every material portion of air, the jet at E was projected ſeveral inches from the adjutage, but its curve decreaſed during the deſcent of more air; for the bubbles did not riſe inceſſantly into the bottle, but after ſhort intervals of reſt, diſlodging two or three ounces of water each time, with a guggling noiſe, which was very audible to the perſon regulating the ſupply of the ciſtern. After the water in the veſſel C was depreſſed to a level with the ends of the pipes, the denſe air carried down A B, aſcended through D E, and cauſed frequent interruptions in the jet; for, expanding under a light preſſure, it expelled the water in the higheſt part of the pipe with violence, and then the efflux ceaſed for ſome time after.

and this air is afterwards uſed for raiſing water.

The condenſed air, however, could any time be let out, by a ſmall pipe which was placed within the bottle, and opened on the outſide of the veſſel C.

The pipe A B had a joint above the bottom of the ciſtern, to facilitate the trial of mouth-pieces of various forms, to find by which the apparatus would ſip the moſt air: and it appeared that no form, or poſition, conduced more to this effect, than when the pipe was crooked at top to receive the water in a horizontal current, and the higher ſide of its orifice was not more than two lines below the ſurface of the water in the ciſtern. It alſo appeared, that no leſs quantity of air was collected, when the diameter of the orifice of emiſſion was reduced to four lines, than when it was half an inch.

Experiments to ſhew how much air can be carried down with he water.

After ſeveral experiments to determine the quantity of water requiſite to ſupply the expenditure from the ciſtern, and keep the ſurface of the fluid accurately at the height beſt adapted to the operation of the apparatus; ſeveral trials were made to aſcertain the quantity of air a given quantity of water would convey into the bottle in a given time. The reſults of ſeveral trials on the 21ſt of May 1803, were as follow:

1. The fall being 50 inches, and the orifice of emiſſion four lines in diameter, the inverted bottle above C, holding ten ounce meaſures of water, was filled with air, under the preſſure of a column 18 feet high, by 14 pints of water flowing out at the orifice of emiſſion at E, in 143 ſeconds. 2. By 13 pints, in 133 ſeconds. 3. By $12\frac{1}{2}$ pints, in 125 ſeconds. By 12 pints. 5. By 11 pits, in 95 ſeconds. 6. By 14 pints, in 114 ſeconds. 7. By 12 pints, in 102 ſeconds. 8. By 12 pints, in 108 ſeconds.

9. The orifice of emiſſion being half an inch in diameter, the bottle was filled by 12 pints. 10. By 13 pints, in 133 ſeconds. 11. By $12\frac{3}{4}$ pints.

12, 13. The fall being 44 inches, the orifice of emiſſion four lines in diameter, 11 pints filled the bottle in 95 ſeconds; and 14 pints, in 120 ſeconds.

With an height of 18 feet and fall 50 inches, 20 parts of water carried down one of condenſed air.

The difference in the time, and the quantity of effluent water required to fill the bottle with air, in theſe trials, was probably occaſioned by a portion of the air being ſometimes contained in the higher, and at other times in the lower part of the pipe A B, at the commencement of the effuſion: or, perhaps,

perhaps, in part, by the water in the ciſtern not being always of the ſame height; for the ciſtern did not overflow, but was ſupplied with great care, ſometimes by a pump, and ſometimes by letting water out of a veſſel, always keeping the ſupply from agitating the contents of the ciſtern as much as poſſible. Had the bottle been larger, there had probably been more uniformity in the reſults of the trials. In eſtimation, I think, however, we ſhall not overrate the operation of this machine, by taking 13 pints for the mean quantity of effluent water emitted while the bottle was filling with air; and then deducting the quantity expelled from the bottle, it will appear that 20 parts of water carried one of air down the pipe A B: and as one ounce meaſure of condenſed air at leaſt was collected in 14 ſeconds, ſo 16 pints would collect every hour.

Trial with a greater length of ſyphon:

Some few days after theſe experiments, the pipe A B was lengthened to 24 feet 7 inches, and D E to 20 feet; but upon trial, the air was carried into the bottle ſo much ſlower than before, that a ſuſpicion aroſe that ſome part of the apparatus was not air-tight; and on this ſuppoſition the pipes were taken down.

Leſs air was carried down.

In February 1804, the pipes, &c. were examined, and ſet up again with conſiderable care. A B was 24 feet 7 inches long; D E, 21 feet one inch; conſequently the difference for the fall was 3 feet 6 inches. With this apparatus, when the diameter of the higher orifice of the pipe D E was four lines, it appeared by four trials (Feb. 25, 1804), that the bottle loſt only one ounce of water per minute.

When the pipe D E was ſhortened to 19 feet 7 inches, and had its higher orifice five feet below the ſurface of the water in the ciſtern, four ounce meaſures of condenſed air deſcended into the bottle, during the emiſſion of 16 pints of water, through the orifice at E, when half an inch in diameter.

The diminution in the collection of air, in theſe laſt experiments, was much more conſiderable than was expected to happen, either from the abſorption of the water, or the increaſed condenſation of the air, which might be occaſioned by ſo ſmall an addition being made to the apparatus. The jet at E was projected more ſteadily in theſe laſt, than in the preceding trials; and the condenſed air, inſtead of riſing into the bottle in large detached bubbles, aſcended in a continual ſtream, like the evolution of gas from the bottom of an effervеſcing

mixture. From the minute diviſion of the air, it is not improbable, that a ſmall portion might be carried along with the current of water under the bottle, and aſcend through the pipe D E; but this was not determined. If there was no defect in the apparatus, it appears, that it will not be ſo advantageous, in the conſtruction of a working machine upon this principle, to employ a condenſing column ſo heavy as 24 feet, as one that is lighter.

Other trials.

Feb. 28, 1804. The pipe A B being ſhortened to 22 feet 5 inches, and D E to 18 feet 3 inches, the orifice of the adjutage at E being four lines in diameter, the bottle was emptied by 13, 12½, 14, 13, 13, 12, and 14 pints of effluent water, in ſeven ſucceſſive trials, as in thoſe of May, 1803.

March 9. With a fall 3 feet 9 inches, A B being 15 feet one inch, half a pint of air was collected during the diſcharge of 12½ pints of water. Again, the orifice of emiſſion being four lines in diameter; the fall 4 feet 2 inches; A B 15 feet 8 inches; D E 11 feet 6 inches; nine ounce meaſures of air were conveyed into the bottle, in one minute, during the diſcharge of 10 pints of water, in five ſucceſſive trials: and when the diameter of the higher orifice of the pipe D E was half an inch, the ſame quantity of air was carried down in 50 ſeconds, by nine pints of effluent water, including that diſplaced from the bottle. When A B was 15 feet 8 inches; D E 13 feet 2 inches; the fall 2 feet 6 inches; nine ounce meaſures of air were collected in the bottle, by the diſcharge of 16 pints, in 90 ſeconds; in 105 ſeconds, by 18 pints; and again, by 18 pints, in 90 ſeconds.

March 16. The fall being 2 feet; A B 8 feet 5 inches; D E 6 feet 5 inches; and the diameter of its higher orifice half an inch; ten ounce meaſures of air were collected in one minute; again in 64 ſeconds, when the effluent water meaſured 10 pints; and again in one minute, when 10 pints.

Having now ſhewn what power a machine operating upon this principle may be expected to poſſeſs, I proceed to ſhew how its principle may be applied to practice.

Deſcription of the machine as conſtructed for raiſing water, by air condenſed in an inverted ſyphon.

Fig. 2, *Plate I.* exhibits a machine for raiſing water above the ciſtern.

R repreſents a ciſtern ſupplied by a ſpring, where there are four or five feet fall for the water.

W W,

Deſcription of the machine as conſtructed for raiſing water, by air condenſed in an inverted ſyphon.

W W, a well or pit ſituated below the bed or lower level of the ſtreamlet; its depth varying from 6 to 20 feet, according to the elevation to which water is to be raiſed above the ciſtern, and the number of progreſſive columns by which it is to aſcend.

A A, a pipe leading from the ciſtern R, to a bell-ſhaped veſſel B, fixed a little above the bottom of the well, with its mouth downwards. The top of the pipe is crooked, as repreſented at A, *Fig.* 5, and there is a joint below, which allows the crooked part to be detached from the reſt. The lower end of this pipe is alſo crooked, and turns under the ſide of the veſſel B.

C C, a pipe fixed into the top of the veſſel B, and carried a little above the ciſtern, where two ſmaller pipes, E and G, are connected with it by a ſtop-cock.

E, a ſmall pipe leading to F, a veſſel or chamber, placed in the ciſtern R.

G, another ſmall pipe leading to H, a veſſel or chamber, ſomewhat leſs than F, placed in a higher ſituation. This pipe has a turn, a foot above the top of the veſſel H.

I, a pipe leading from the ciſtern R to the veſſel F.

K K, a pipe deſcending a foot or more below the veſſel F, and then aſcending to the veſſel H.

L, a pipe connected to K, a foot below H, and then carried to the conduit or ciſtern which receives the raiſed water.

The pipes I, K, L, have each a valve opening upwards.

The conſtruction of the cock is repreſented in *Fig.* 3. The conical barrel has four holes, C, E, G, O, and the turning part or key, has a notch, or hollow, on each ſide in that part which moves oppoſite thoſe holes, ſo that the pipes C and E, or C and G, may be connected by a quarter of a turn. When the communication opens between C and E, the external air has acceſs to the inſide of the pipe G, and to the chamber H above, through the opening O; and when C is joined to G, the air is admitted into the chamber F, through the opening O, and the pipe E.

In the narroweſt part of the veſſel B, a buoy or float is fixed upon the elevated end of a crooked lever, moving upon a horizontal axis, ſupported by pieces attached to the ſide of the veſſel. Inſtead, however, of a common hollow buoy, it will be preferable to uſe a body ſpecifically heavier than water, and

by

Deſcription of the machine as conſtructed for raiſing water, by air condenſed in an inverted ſyphon.

by adding weight to the other end of the lever, to make ſuch adjuſtment, that while both are immerſed in water, the body within the veſſel B, ſhall be a few ounces lighter; and, on the contrary, when it alone is above the water, it ſhall be ſo much heavier than its counterpoiſe, which is covered. A ſmall cylindrical veſſel $2\frac{1}{2}$ inches in diameter, and the ſame in depth, filled with water and cloſed, will probably be ſize ſufficient for ſuch a float; and the proper counterpoiſe may be very readily and eaſily found, by having the lever fixed on its axis in a veſſel of water, repeatedly drawing off and replacing the fluid in that part of the veſſel which contains the float; and increaſing or diminiſhing the weight, until the proper adjuſtment is obtained. But to proceed with our deſcription.

S repreſents a ſmall ſyphon ſuſpended by a lever, with one branch in the inſide, and the other on the outſide of the ciſtern R. The outſide branch being re-curved in the manner repreſented in *Fig.* 5, it is evident, that when the inſtrument is filled, it will draw water out of the ciſtern whenever the orifice of the re-curved branch is depreſſed below the level of the water in the ciſtern; and that its operation will be ſuſpended by raiſing the ſame a very little above that level. From the contrary end of the lever, a chain or wire deſcends, and is connected to the lever which carries the float; and by this connection, the ſyphon is ſuſpended with the orifice of its re-curved branch above the ſurface of the water in the ciſtern, while the float occupies the higheſt part of the veſſel B; for the weight of the ſyphon and that of the included column muſt be ſo nearly counterpoiſed by the chain and an additional weight, that it cannot depreſs the float, though it muſt poſſeſs ſufficient weight to deſcend when allowed by the deſcent of the float.

M repreſents two cuneiform buckets, connected at their baſes by a tranſverſe partition, and fixed upon a horizontal axis, as is more clearly exhibited by the ſection, *Fig.* 4. When the bottoms of theſe are placed in an oblique direction, making an angle with the horizon of 25 or 30 degrees, as repreſented in the drawing, (*Fig.* 2.) and a ſmall ſtream of water falls from the ſyphon, the higher bucket will receive the water, and falling in conſequence of its load, will raiſe the other bucket, which will now receive the water, and by falling will raiſe the firſt, whoſe contents were emptied in its deſcent.

descent. A small arm or lever is fixed to the axis of this apparatus, and connected by a piece of strong wire to another shorter lever fixed upon the smaller end of the turning part of the cock C, and by this means the cock moves with the alternate motion of the buckets M, when supplied with water.

Description of the machine as constructed for raising water, by air condense in an inverted syphon.

When the space from the top of the vessel B, to the surface of the well is equal to 18 feet, the top of the chamber H, above the bottom of F, and the perpendicular height of the pipe L, above the bottom of the chamber K, may be each 18 feet.

The valves and every other part of the apparatus being in proper order for work; the well being filled with water; and the reservoir R constantly supplied, so as to overflow in one part somewhat lower than the rest of the brim, while the higher orifice of the pipe A A, is about two lines below the surface of the water, and takes in its full quantity; the manner of bringing this machine into action, and its operation afterwards, may be understood by attending to the following directions, and statement of particulars:

Open the pipe E which leads to the chamber F, by turning the cock C, and water will descend into F from the cistern R, by the pipe I: When the chamber is full, place the two connected buckets M, in a horizontal position, and the cock C, if properly constructed and connected with these buckets, will cut off all communication between the pipe C C and the pipes E G. The air carried down the pipe A A, by the column of water which descends and keeps the well constantly overflowing, will ascend into, and gradually expel a quantity of water from the pipe C C, and afterwards that contained in the higher part of the vessel B also. The float receiving an accession of weight by being out of water, will descend and let down the re-curved syphon S, which will pour water upon the buckets below. At this period depress that bucket which by descending opens a communication between the pipes C and E, and no farther attendance will be requisite.

The pressure of the column in the well, above 18 feet high, being thrown upon the water in the chamber F, by the intervention of the condensed air in the pipe C, the valve in the pipe I will be shut, and water will ascend from the chamber F through the pipe K K into H; the syphon S will also ascend into its place, and cease to draw water from the cistern.

Deſcription of the machine as conſtructed for raiſing water, by air condenſed in an inverted ſyphon.

ciſtern. An equilibrium being ſoon eſtabliſhed between the acting column in the well, and the re-acting one included in the pipe K K and the chambers F, H, by the expulſion of the air from B, the water will afterwards aſcend much ſlower into H than at firſt, an equal depreſſion of the fluid being produced in the pipe C and the chamber F, by the collecting of the condenſed air.

In more or leſs time, according to the capacities of the chambers, ſo much water will be expelled from the lower as will fill the higher, the air having been expelled from this laſt, through the pipe G, whoſe outlet is at O in the cock C E G O, *Fig.* 3. Water will then begin to aſcend into that part of the pipe G which riſes from the top of the veſſel, and the acting column being lengthened in proportion to the increaſing height of that which it counterpoiſes, the condenſed air will depreſs the water in the veſſel B below the float, which deſcending, will lower the re-curved ſyphon, and water will fall into the elevated bucket M, which ſoon afterwards in conſequence of its load will deſcend, and by moving the cock above, will open a communication between the pipes C and G, and between the inſide of the chamber F and the external air, when the condenſed air will ruſh out, and this veſſel refill with water from the ciſtern.

The force of the acting column being now thrown upon the contents of the higher chamber H, the valve in the pipe K will cloſe, and water will aſcend through the pipe L, into the ciſtern appropriated for its reception.

At the firſt opening of the communication between B and H, the re-curved ſyphon S will pour water into the bucket laſt elevated; but before the load is ſufficient to move the apparatus, the ſyphon, if properly adapted to its purpoſe, will be drawn up again, in conſequence of the condenſed air being expelled faſter from the veſſel B, than it deſcends by the pipe A A: for the water will always riſe with the greateſt rapidity after the turning of the cock, becauſe of the difference ſubſiſting between the acting and re-acting columns, and the air previouſly ſtored up in the veſſel B. The ſupply of condenſed air, however, being inadequate to ſupport the difference, an equilibrium ſoon takes place, by the water aſcending into the loweſt part of the pipe C.

The

The ſupply of condenſed air will now continue to force water out of the chamber H, and to depreſs the fluid in the pipe C, in an equal degree, until all the water in the chamber is expelled. The water will then ſink in the higheſt part of the pipe K, and the acting and re-acting columns proportionally lengthening, air will collect in the veſſel B; the float and re-curved ſyphon will deſcend; and before the depreſſion of water in the pipe K reaches the lower end of the pipe L; before the condenſed air can eſcape by expelling the water contained in this pipe, the elevated bucket M will fall in conſequence of water poured into it by the ſyphon, and the communication between C and E being opened, the condenſed air will ruſh out of H, through the open pipe G, whoſe outlet is at O in the cock C E G O, *Fig.* 3; the valve in L will ſupport the water above it, and the force of the acting column being again thrown upon the water in the chamber F, the fluid will begin to aſcend into K; the re-curved ſyphon will riſe and continue in its place as before, until the water begins to fill that part of the pipe G which riſes from the top of the chamber H, or, if there cannot be a ſufficient quantity raiſed from the lower chamber to fill the higher, until it begins to be depreſſed into that part of the pipe K, connected with the bottom of the chamber F; the ſyphon will then be let down by the float, will pour water into the bucket laſt elevated, and thus again open the communication between the air-holder B C, and the higher chamber, from which the water will be expelled; and in this manner the alternations will proceed, the machine continually raiſing water from one or other of its chambers.

Deſcription of the machine as conſtructed for raiſing water, by air condenſed in an inverted ſyphon.

By additional pipes and chambers, ſimilar to G H L, a ſmaller quantity of water may be raiſed ſtill higher, the loweſt additional chamber being fixed upon the top of the pipe L, and the pipe for ſupplying it with condenſed air, connected to the pipe E. A pipe for ſupplying a chamber ſtill higher muſt be attached to the higheſt part of the pipe G; and it is evident, that if E leads to two chambers and G only to one, the machine will regulate its operations ſo as to loſe no time. It will be requiſite, however, to fill the additional chambers with water before the machine is ſet to work.

Where the ſupply of water is variable, the machine may be adapted by having ſeveral pipes ſimilar to A A, but ſome wider

Deſcription of the machine as conſtructed for raiſing water, by air condenſed in an inverted ſyphon.

wider and others ſmaller, and by ſetting ſuch of theſe to work as are requiſite, for whatever may be the increaſe or diminution of power, the turning of the cock will be duly regulated by the float.

The water which ſupplies the pipe I, and aſcends in the apparatus, ſhould be cleared, by filtration, from impurities and ſubſtances that would obſtruct the cloſure of the valves. The ciſtern ſhould have a moveable piece at the place where the water overflows, to accommodate the ſurface of the fluid to the ends of the pipes, that the full quantity of air may deſcend with each column, and that the maximum of effect may be obtained from the ſupply.

To determine how well ſuch a combination as that I have deſcribed would anſwer the purpoſe, I had a model conſtructed upon the plan exhibited in *Plate I. Fig. 2*; but having no convenience for an overflowing well or ciſtern, I was obliged to modify ſome parts, in a manner tending to diminiſh its power.

The water from the ciſtern R, falls into a capacious veſſel, from whence, when the machine is at work, a hand-pump continually raiſes it again into a veſſel above, which ſupplies the ciſtern R through a pipe nearly half an inch in diameter, under a conſtant preſſure of $3\frac{1}{2}$ inches charge. The ſupply keeps the ciſtern continually overflowing, and the ſurface of the water is calm and always at the ſame height.

The pipe A A is 8 feet 3 inches in length, and half an inch in diameter. Its lower end is inſerted into the veſſel B, which is cloſed at bottom, and conſtructed of ſuch a form as to include the lever which carries the float. Above that end of the lever bearing the counterpoiſing weight, a vertical pipe 6 feet 3 inches, is connected to the top of the veſſel; and through this pipe, which is no wider than A, the water aſcends and flows to the pump: A chain, conſiſting of pieces of wire four or five inches in length, looped together by the ends, alſo paſſes through it, and connects the float-lever to that which moves the re-curved ſyphon S.

The bottoms of the buckets M are both together 14 inches long and 6 broad. The baſe partition is 4 inches high. Each bucket has an end parallel with the baſe, one inch deep, provided with a hole to let out the water when depreſſed. The cock moves with leſs than a pint of water in the elevated bucket.

The

Deſcription of the machine as conſtructed for raiſing water, by air condenſe in an inverted ſyphon.

The float in the veſſel B, is a ſmall cylindrical copper veſſel, one inch in height and two inches in diameter. It was filled with water and cloſed before it was fixed in its place. Though equal in bulk only to one ounce and three quarters of water, yet it is quite ſufficient to move the ſyphon which would work a larger machine.

The pipe C is half an inch in diameter: E, K, G, L, are ſmaller. The valves are leather.

This machine, when in good order, raiſes water nearly 12 feet above the ciſtern, at the rate of $20\frac{1}{2}$ pints per hour, and performs all its operations as well as can be deſired. When firſt ſet to work, the cock is ſo placed as to cloſe the top of the pipe C, until the condenſed air begins to collect in the veſſel B, and then the communication is opened to the chamber F. If the communication was open at firſt, the water would be expelled from F into the ciſtern, while the preſſure was inſufficient to cloſe the valve in the pipe I.

The chambers F H being ſmall, the ſyphon moves frequently; but in a working machine theſe veſſels ſhould not only be broad and ſhallow, but capacious, that the wear of machinery may be reduced to its utmoſt extent.

To determine what quantity of water flows through the apparatus, I fixed a ſpout upon the top of the aſcending water pipe; but in doing this I entangled the float in the veſſel B, that it could not be made to work the ſyphon. The effluent water, in this unemployed ſtate of the machine, including half a pint diſplaced from the veſſel B, amounted to $8\frac{1}{2}$ pints. The ſuperfluous water from the ciſtern R meaſured $8\frac{1}{2}$ pints alſo. If the machine had been working the waſte water would have been leſs, as part would have been drawn off by the ſyphon.

From ſeveral trials, this model appears to raiſe water above the rate that might be eſtimated by the experiment of March 6, previouſly related.

In actual practice, I think the allowance for waſte and working the buckets, of one third or perhaps only of one fourth of the ſupply, will be ſufficient; then ſuppoſing the apparatus ſo adapted to the ſupply, as 28 or 30 gallons from the ciſtern will raiſe one gallon 18 feet, ſo 84 or 90 gallons will raiſe one gallon 44 feet, by three aſcending columns.

The

The bucket-engine at Irton-Hall, in Cumberland *, is ſaid to raiſe one gallon of water 60 feet high by 36 gallons ſupply; hence, if the waſte water be included, it appears that our machine will not be competent to perform half as much work by the ſame ſupply, and its peculiar advantages muſt depend upon its durability when conſtantly employed.

I am, Sir,

Your's reſpectfully,

WILLIAM CLOSE.

V.

Remarks on COUNT RUMFORD'S *Experiments relating to the Maximum Denſity of Water. In a Letter from Mr.* JOHN DALTON.

To Mr. NICHOLSON;

SIR,

Count Rumford's experiments on the max. denſity of water conſidered.

IN your laſt Number, page 225, is an intereſting article on the queſtion, At what point of temperature water is of greateſt denſity? From the introductory paragraph I was led to expect, that all the material objections to the current doctrine were conſidered and obviated, and that new and convincing arguments in its ſupport would be adduced. In the former of theſe expectations I was altogether diſappointed; and though the new experiments are ingenious and well worth attention, they are not quite ſo demonſtrative to me as they appear to be to Count Rumford. Perhaps we may both be too ſtrongly biaſſed towards preconceived theories: however this may be, it ſeems proper that when new facts are brought forward, we ought to reconcile them to the theory eſpouſed.

Mr. Dalton's exper. in this Journal on the ſame ſubject.

At page 93 of Vol. X. of this Journal, I have ſtated a number of facts and experiments which appear to me irreconcileable with the notion of water being denſeſt at 40°. I believe it is denſeſt at 32°, or the freezing point; and it is my preſent intention to ſhew how, on my hypotheſis, I explain Count R.'s reſults.

* See Philoſ. Journal, Vol. II. p. 60.

Water

Count Rumford's experiments explained, on Mr. Dalton's hypotheſis.

Water has very little change of dimenſions by heat or cold near its max. denſity.

Water expands by heat from ſome point (whatever it may prove to be) by a law which is nearly that of the ſquare of the temperature from the ſaid point, as is evident from Sir Charles Blagden's table. Conſequently the force of aſcent, which water acquires by temperature, is at firſt very ſmall, but increaſes to a very conſiderable amount before ebullition. The coheſion of the particles of water is a conſtant force; there will therefore be a point of equilibrium between theſe two forces; that is, a point at which the increaſed temperature will be but juſt ſufficient to counteract the tenacity, in which caſe no internal motion can enſue. Whether a diminution in denſity in water to the amount of *one hundredth* or *one thouſandth*, or *one ten thouſandth* or more, is the point alluded to, is to be determined only by experiment. I apprehend that water of 40° is about one *ten thouſandth* part lighter than water of 32°; but that this force of aſcent is but juſt ſufficient to counteract the tenacity, and conſequently no motion takes place; in ſuch caſe the diffuſion of heat through water is the ſame as through a ſolid body. Whenever the difference in denſity exceeds that juſt mentioned, internal motion is the conſequence, and that greater in proportion to the difference of denſity, which we know may amount to $\frac{1}{24}$ of the whole.

This change at 40° is not ſufficient to produce a current;

—and therefore the heat at firſt paſſed downwards as if thro' a ſolid; but in higher temperatures there was a current, which prevented the deſcent.

Count R.'s experiments therefore will be explained by obſerving that the thermometer acquired heat by the proper conducting power of water, as if it had been metal, or any other ſolid body; the temperature acquired was greater in the 2d experiment than in the 1ſt, becauſe the heat of the ball was greater; but in the 3d experiment the heat of the ball was ſuch as to produce a current upwards that almoſt precluded the deſcent of heat, by carrying away the heated particles as ſoon as formed.

Difficulty reſpecting the two thermometers.

The circumſtances of the two thermometers by the ſide of the ball and the cup, in the two firſt experiments not acquiring any temperature, is certainly remarkable, and not eaſy to be explained, even upon Count R.'s principle; for, the ſuppoſed deſcending current of warm water ſhould have filled the cup and overflowed, ſo as to affect the collateral thermometer.

Important experiment propoſed. Let the water be at 40° and the ſolid at 32°.

One moſt important experiment Count R. has omitted, and which it is particularly deſirable that he, or ſome one in poſſeſſion of a ſimilar apparatus, would perform, eſpecially as it would go further than any other to eſtabliſh the doctrine of *cur-*

rents

rents in water, when the temperature varies from 32 to 40°. This is to repeat the 1ſt experiment, with the difference that the maſs of water ſhould be at the temperature 40°, and the ball at 32°; in which caſe the thermometer in the cup would not be at all affected upon Count R.'s principle; but if the explanation I have attempted above be accurate, no material difference in the reſults of the two experiments would be obſerved.

I am your's,

J. DALTON.

Mancheſter, Auguſt 17, 1805.

VI.

On the Art of bending Wood. By J. H. HASSENFRATZ.*

Either live or dead wood may be bended.

THE operation of bending may be performed either on live or dead wood, the proceſſes differing however for theſe two ſtates.

I. *On the bending of Live Wood.*

Live wood may be

Live wood has a natural elaſticity †, which varies according to its ſpecies, ſize, and age. The larger and older the wood, the leſs elaſticity it poſſeſſes.

This operation is performed on wood when growing, either to ſtraighten it, to give it a figure ſuitable to the ornamental purpoſe for which it is deſigned, or to ſhape it to the uſe for which the timber is intended when cut. Thus trees may be bended, which are intended for the building of ſhips, or for making the felloes of wheels in one piece.

—bended for ſhip-building, or for felloes of wheels, &c.

By faſtening down young trees.

When trees are yet young and pliable, their ſtems are faſtened down by ropes, or poles, or ſtakes, or frames. In this ſituation they are confined till they will retain when let looſe the curvature that has been given them.

This the moſt eaſy proceſs;

Of all the modes of bending timber the moſt eaſy and commodious is that applied to young growing trees: for their pli-

* Tranſlated from the Journal des Mines, N. 94, p. 475, July, 1804.

† In many parts of this paper the writer ſeems to have uſed the word elaſticité for the property of undergoing flexure without breaking. T.

antneſs

antneſs and elaſticity enable them to acquire any form that may be deſired; ſo that there are few to which the moſt whimſical figures may not be given, with due care and the requiſite precautions; but at the ſame time we injure their natural conſtitution, retard their progreſs, and frequently reduce them to a ſtate of conſtraint and diſeaſe prejudicial to their growth.

but injurious to the timber.

II. *Of the bending of Dead Wood.*

The bending of dead wood is moſt advantageous.

The bending of wood that is cut down and dead, though more difficult, is yet more in uſe, becauſe we may chooſe ſuch as is beſt adapted to the purpoſe for which it is deſigned, and then give it the ſuitable curvature.

The principle is the action of heat, which increaſes the pliableneſs of timber.

The proceſs generally employed is founded on the property caloric poſſeſſes of augmenting the elaſticity of wood by penetrating it, and diminiſhing this elaſticity on quitting it.

Thus when we wiſh to bend thin pieces, as the ſtaves of barrels, or the planks of boats, we heat them at the part that is to be curved, and bend them gradually as they grow hot.

Partial heat affects the wood unequally, and occaſions it to crack or ſplinter.

But heat applied to one part of the wood, while the other is in contact with the air, heats it unequally, and increaſes the pliableneſs but partially; ſo that on bending it, ſome parts are ſtiff and others yield, occaſioning an unequal curvature, and ſometimes cracks or ſplinters in the inſide or on the ſurface of the wood. The only method of remedying this inequality is to heat the wood equally throughout.

Furnaces or ſtoves heat it uniformly, but may ſcorch it.

Furnaces or ſtoves gradually heated are adapted to the purpoſe of affording a uniform heat, and conſequently facilitating the curvature of the wood; but in uſing them there is reaſon to fear, that the caloric, while heating the wood, may expel from it the fluids contained in it, char it, and wholly deſtroy its elaſticity.

Humidity as well as heat neceſſary to render wood pliable.

The pliableneſs of wood is in proportion not to its temperature alone, but to its humidity likewiſe. The ſame wood at the ſame temperature will be more or leſs pliable in proportion to the water contained in it; and at an equal degree of moiſture its elaſticity will be proportional to its temperature.

The ſingular ſtructure of a mortiſe and tenon.

We have an inſtance of the double influence of heat and moiſture in joining two pieces of wood with a tenon and mortiſe, where the mortiſe is only a third of the breadth of the piece that is driven into it to form the joint. Theſe joints, ſo extraordinary in appearance, ſurpriſe people ſo much, that

Heat and moiſture are applied to bend timber;

moſt of thoſe who uſe them make a myſtery of them. The proceſs employed in this operation has given riſe to the method at preſent in uſe for bending with eaſe the largeſt and ſtiffeſt timber: it conſiſts in penetrating it with humidity, and at the ſame time imparting to it a uniform temperature, then bending it, and letting it cool, while it is kept in the form to which it has been brought.

—in three different ways:

For heating and moiſtening the timber, three different proceſſes have been employed: firſt, boiling water; ſecondly, ſteam; the third, wet ſand heated.

1ſt. By boiling water.

The ſtove for the firſt proceſs conſiſts of a large copper boiler, heated by three furnaces, cloſed by a movable cover, and varying in its dimenſions according to the ſize of the timber for which it is intended. Cranes are uſed for raiſing the timber, and putting it in or taking it out of the boiler, which is kept full of water. When the timber is in, the cover is put on and beaten down cloſe, to diminiſh the evaporation of the water; the three fires make the water boil, the timber is heated and penetrated with moiſture, and it is then taken out to be bent.

This diſſolves ſome of the component parts, and leſſens the dimenſions of the wood.

This proceſs, one of the firſt that was employed, has the defect of diſſolving a part of the proper ſubſtance of the wood in the boiling water; the timber ſhrinks in drying, ſo as to become narrower and ſhorter; its ſtrength and elaſticity are conſiderably diminiſhed, and from theſe alterations occaſioned by it the proceſs is diſuſed.

2d. By ſteam. Deſcription of the ſteamer.

Figures 2, 3, and 4, Pl. II. repreſent the plan and elevations of a ſteamer. It conſiſts of a large wooden box, formed of ſtout planks, held firmly together by ſquare frames. Within are ſupports for the timber that is to be expoſed to the action of the ſteam. The dimenſions of the box are regulated by the ſize and quantity of the wood intended to be ſoftened.

For ſmall ſteamers a boiler is fixed at one extremity of the wooden box, and the wood is introduced at the other through an opening, the door of which either ſlides in a groove or turns on hinges. For large ones the boiler is fixed in the centre, and there is an opening for the timber at each end. In the ſide oppoſite the boilers are openings *a a a* for arranging the timber on the ſupports. It is uſual to leave the wooden boxes expoſed to the air externally; but it would be of advantage to

It ſhould be covered with ſome bad conductor of heat.

cover

cover the planks with ſome ſubſtance that is a bad conductor of heat, to confine the heat that is diſengaged from the ſteam within.

Each boiler having a communication with the interior of the box, by means of a pipe, the ſteam is diſtributed to each ſtage by the tubes *b b b*, *Fig.* 3. The vapour ariſing from the boiling water penetrates the timber with moiſture, heats it, increaſes its elaſticity, and renders it fit to be bent.

Steamers require little care, and little expenſe, but they cannot be uſed for timber of any great thickneſs, ſince they cannot impart a temperature higher than that of boiling water, and this is not ſufficient to give large pieces the degree of pliableneſs neceſſary for bending them.

This proceſs is eaſy in uſe, and not expenſive; but it is not hot enough for large timber.

This lowneſs of temperature gave riſe to the invention of the ſand-ſtove, which is formed of four ſtone or brick walls. In the middle are two furnaces, with which ſeveral circular flues communicate, for conveying the heat, the heated air, and the ſmoke, to a chimney riſing from each end. On theſe flues are plates of caſt iron, which form the bottom of the cavity in which the ſand is placed; the flame and ſmoke circulating in the flues heat theſe plates, and theſe plates heat the ſand. This is an imitation of thoſe ſand-baths which have been long employed in many chemical proceſſes and in ſeveral manufactories.

3d. By wet ſand. Sand-ſtove deſcribed.

As the ſand may be heated to a temperature above that of boiling water, it can communicate a greater heat to the timber; but were there nothing but ſand and timber in the ſtove, all the gaſeſiable ſubſtances in the timber might be expelled by the heat, and the timber charred.

To prevent this, one or two boilers filled with water are placed in the middle of the ſtove. The water converted into ſteam by boiling penetrates the ſand with moiſture; this imparts moiſture to the timber; and thus the heat that pervades the timber expels from it no more moiſture than is replaced by the ſand, ſo that all the proper ſubſtances of the timber are preſerved.

Steam muſt be uſed with the ſand heat;

We will not venture to affirm however, that no portion of the component parts of the wood is evaporated in this operation, and that conſequently it undergoes no alteration; but with the precaution of taking out the wood to bend it as ſoon as it is ſufficiently heated and penetrated with moiſture, the injury is imperceptible.

and the timber will not be perceptibly injured.

The ſand-ſtove is covered throughout its whole length, to retard the evaporation of the moiſture contained in it, and allow the heat to accumulate ſufficiently to give the wood the proper temperature.

Manipulation for the heat.

The pieces of timber are introduced at the ends, placed in the middle of the ſtove in the direction of its length on bars fixed for the purpoſe, and covered with ſand.

When the timber has been heated and penetrated with moiſture to the proper degree for enabling it to aſſume the degree of curvature required, it is bent to a line deſignating the curve.

The wood may be bended horizontally or vertically,

The timber may be bent in two ways, either horizontally or vertically; the former is uſed for pieces of ſmaller dimenſions and greater curvature.

—by any mechanic power;

In either way the force that produces the curve is applied by means of ropes, tackles, or even capſtans. The piece muſt be kept in the ſhape to which it is brought, and thus left to dry and grow cold, when it will retain the curvature given to it.

--which may be applied various ways.

Frequently when the piece of wood is thin, preſſure by hand, or by weights, will bend it ſufficiently, ſo that it will retain its ſhape on cooling. But the means of bending it may be varied to infinity, according to the elaſticity of the timber, its ſize, its temperature, and its humidity.

VII.

Experiments made in the great, in a reverberating Furnace on Caſt Iron, confirming the eſtabliſhed Theory reſpecting the Difference between caſt and malleable Iron. *By* G. A. Lampadius, *Prof. of Chemiſtry and Metallurgy at Freyberg.**

The reverberatory furnace deſcribed.

I SHALL firſt deſcribe the reverberatory furnace uſed in theſe experiments. It had three principal parts: 1. the air tunnel

* Extracted by J. F. Daubruſſon, in the J. des Mines, from the *Sammlung Practiſch-chemiſcher Abhandlungen* 'Practical Chemical Eſſays,' of Lampadius, Vol. II. p. 145.

Prize queſtion of Bohemian Society, 1795.

In 1795, the Royal Society of Bohemia propoſed as a prize queſtion to ſettle the theory of the refining of iron, taking as a baſis the labours of Vandermonde, Berthollet, and Monge, on the different ſtates

tunnel and aſh-hole; 2. the fire-place; 3. the hearth and chimney. To obtain the proper degree of heat, the air was conducted through a vertical tunnel ſeveral ells long (the Saxon ell is near two Engliſh feet), the lower aperture of which was over a ſtream of water, and conſequently it brought rapidly to the fire-place a ſupply of freſh and condenſed air. The fuel was wood; the bottom of the furnace was an oval cavity, covered with a heavy coating, and capable of containing three or four hundred weight of metal. The flame, which traverſed the furnace with rapidity, eſcaped afterwards through a chimney eight ells high. The furnace had an opening capable of being cloſed at pleaſure by an iron door. There was another above the fire-place, a few inches ſquare, ſerving to admit the nozzle of a pair of bellows, or the neck of a retort.

It was an air furnace having an hearth within and its chimney was 16 feet high.

In the uſe we made of this furnace I had an opportunity of obſerving very diſtinctly, that in the flame of a cloſed reverberatory furnace there are always a multitude of unoxided particles of carbon, which impart to it the capability of reducing (diſoxiding) metal. This opinion I had already announced on occaſion of a memoir of Mr. Dacamdra. In ſome of our trials, making uſe of the wood of the Scotch fir, we obſerved, that the ſmoke iſſuing out was black and denſe, and this the more the freſher the wood; but as ſoon as we made uſe of the bellows, the flame appeared clear, becauſe the oxigen introduced by the air or vapour oxided the carbon that was in the flame, and thus produced a greater heat.

Unoxided particles of carbon viſible in the flame of this cloſed furnace.

Firſt Experiment with the ſimple Fire of the Furnace. Exp. I.

The furnace having been heated for ſome hours, and the fire being very violent, about three hundred weight of metal was taken from the crucible of the high furnace, and poured into the reverberating furnace. This caſt iron, when become ſolid, was gray, and of a fine grain. At the expiration of an hour a frothy ſcoria appeared on the ſurface of the metal, which, to

Gray fine grained caſt iron in the reverberatory furnace, covered with frothy ſcoria, chiefly carburet of iron.

ſtates of iron. Mr. Lampadius ſhared the prize. His memoir may be conſidered in general as a confirmation and ſupplement to the labours of the French academicans; the experiments which he made at Muckenberg in Saxony, in the iron works of Count Von Einſiedel, affording him freſh proofs of this theory. Theſe experiments are here preſented to the reader. D.

judge from appearances, conſiſted chiefly in carburet of iron. We attempted to remove it; but as ſome of the metal adhered to it, and came away at the ſame time, we deſiſted. Soon after, the furnace being cloſed, we heard a continual boiling, reſembling that of a viſcous ſubſtance in a cloſe veſſel. On opening the furnace, we perceived that the whole matter in reality boiled, and that bubbles were continually riſing, which burſt on the ſurface with a beautiful bluiſh flame. Theſe jets of flame had the colour exhibited by carburated hidrogen gas. The boiling continued as long as the fire was kept up; at the ſame time a pretty large quantity of ſcoria was formed, which however could not be removed, on account of the viſcous conſiſtence now acquired by the metal. Beſides, as the metal was frequently ſtirred to preſent a freſh ſurface to the air, the ſcoria mingled with it. At the end of five hours it was no longer fluid, and appeared to be refined. It had loſt its gray colour and fineneſs of grain, was white and coarſe grained, and ſhowed itſelf more malleable, though it was not capable of being forged. The refiner carried it to his ordinary furnace, and there it was refined in leſs time, and required leſs labour than common caſt iron.

Not removable for adhering metal.

The metal was brought to ebullition.

Carburated hidrogen gas evolved.

In five hours it became white and coarſe grained, and a little malleable.

This was afterwards refined ſooner than common caſt iron.

As in this trial we were unable to ſeparate the ſcoria, and no change had been made in the form of the hearth of the common refinery, which ought perhaps to have been done, nothing poſitive can be advanced with reſpect to the practical advantage of refining by the help of reverberatory furnaces; we were merely convinced of its poſſibility, and enabled to demonſtrate the theory of this proceſs, that is to ſay, of ſeeing clearly what paſſed in the operation. The caſt iron was here converted into malleable by means of the oxigen that was in the little atmoſpheric air, which, jointly with azote and carbonic acid gas, covered the metal in fuſion. This oxigen combined with the carburet of iron, and then carbonic acid gas and oxide of iron were formed; this produced the bubbles of air and the ſcoria. The lightneſs of the frothy ſcoria, which aroſe to the ſurface at the beginning, was the reaſon of their ſeparation from the reſt of the maſs; but as ſoon as the air began to act they were deſtroyed.

The proceſs of refining iron in a reverberatory furnace

ſhews that carbon is burned off.

Second

Second Experiment; the Fire of the Furnace being aſſiſted by the Vapour of Water. Exp. II.

The carburet of iron is decompoſed by aqueous vapour.

I had attempted to decompoſe carburet of iron in ſmall quantities by the help of water in the ſtate of vapour. By heating the carburet red hot, the water was decompoſed, and I obtained carbonic acid gas, hidrogen gas, and oxide of iron. As the chief difference between caſt and malleable iron conſiſts in a certain quantity of carburet of iron contained in the former, and which muſt be ſeparated to render the iron malleable, I was deſirous of trying the effect of water in vapour on caſt iron in a reverberatory furnace, principally in order to know how far the refining of iron might be carried on in this way.

Application of this principle to refining iron.

About three hundred weight of caſt iron of the ſame quality as before, and juſt taken from the high furnace, were put into the reverberatory furnace as in the preceding experiment; we then took a large tubulated iron retort, put into it nine or ten quarts of water, fitted a gun barrel to its neck, and introduced the end of the gun-barrel into the little opening in the furnace. The water in the retort was made to boil, ſo that the ſteam diffuſed itſelf with the flame over the melted metal. At the expiration of half an hour all the marks of refining that had been obſerved before were perceptible; the ebullition was conſiderable, and the flame that iſſued from the chimney more bright. Two hours after the commencement of the proceſs freſh water was put into the retort. In about three hours the metal began to thicken, and at the end of four hours it exhibited the marks of refined iron, and we imagined the operation to be finiſhed. We found the grain of this iron finer, however, than that of the iron operated upon in the preceding experiment, and the maſs was full of little blebs.

Steam was introduced, by which the operation proceeded quickly, but the iron was of fine grain, full of blebs,

We gave it to the refiner, who treated it like the former; but to our great aſtoniſhment we found that it wrought worſe in the refinery fire than caſt iron the moſt difficult to refine. It required much more labour, and an hour's time longer.

and was leſs capable of being refined than before.

Having aſſayed a ſpecimen in the ſtate in which it came out of the reverberatory furnace, I found it to contain a much larger quantity of oxigen. Experience had already taught me, that half a pound of gray caſt iron, treated in a retort with four ounces of charcoal from which all carbonic acid gas had been

There was more oxigen in this than in other kinds of caſt iron.

been expelled, gave 32* cubic inches of carbonic acid gas. An equal quantity of white caſt iron afforded 165 cubic inches of the ſame air. Four ounces of the caſt iron juſt taken from the reverberatory furnace, mixed with two ounces of charcoal, yielded 96 inches, or 192 inches to half a pound.

Hence we may infer, that the proportions of oxigen contained in theſe different kinds of caſt iron are,

In iron ſuper-refined by the vapour of water -	192
Common white caſt iron - - - - -	165
Gray caſt iron - - - - - -	96

This ſuper-refined iron imbibed oxigen from the decompoſed water which deſtroyed its carburet.

To the iron produced in the experiment juſt mentioned, I give the epithet ſuper-refined †, becauſe I conceive it to have been formed as follows:—The water in vapour was decompoſed, and deſtroyed the carburet, asatmoſpheric air does in the ordinary refining; but at the ſame time this water imparted to the iron ſo large a quantity of oxigen, that in the refinery it was neceſſary, not only to ſeparate the ſcoria, but to diſoxide the metal likewiſe. This experiment farther confirms the property iron poſſeſſes of becoming oxided in different degrees.

If this experiment afforded nothing practically beneficial, it has at leaſt thrown ſome new light on the properties of caſt iron.

Exp. III. *Third Experiment; the Fire of the Furnace being aſſiſted by the Action of Bellows.*

Bellows applied with the reverberatory furnace.

The ſame furnace was uſed, and the place of the retort in the preceding experiment was ſupplied by a pair of double bellows mounted with leather, 5 feet (4 f. 8 in.) long, 3 (2 f. 10 in.) broad, and 4 (3. f. 9 in.) high at the poſterior extremity when open. It was ſo placed, that the ſtream of air was parallel to the flame and to the middle of the furnace, and worked at the rate of eight or ten ſtrokes in a minute. We were deſirous of ſeeing how far the air thus aſſiſted would carry the refining; the furnace being managed and filled as before.

The heat was much greater.

At the end of half an hour the heat was perceived to be much greater than in the firſt and ſecond experiments. The phenomena of the refining already mentioned appeared in ſuc-

* Probably this is an error of the preſs in the original: as it does not agree with the proportion aſſigned in the next paragraph, one of the two muſt be wrong.

† Or ſurcharged with oxigen.

ceſſion;

ceſſion; but inſtead of the frothy ſcoria obtained in the firſt eſſay, a very fluid ſtratum was formed, which diffuſed itſelf over the melted metal, and prevented its refining. This ſcoria, when grown ſolid, was of a blackiſh brown colour and vitreous fracture. We endeavoured more than once to remove it, but the ſtratum was ſo thin as to render it impracticable: As ſoon as one ſtratum was removed, another formed. At the expiration of four hours, the metal being ſtill very fluid, we began to ſtir it, in order to bring its different parts ſucceſſively into contact with the air; this produced an extraordinary heat in the furnace, combuſtion, and ſcintillation, reſembling that which takes place when iron wire is burnt in oxigen gas. This oxidation always produced freſh ſcoria: as ſoon as we deſiſted from ſtirring, every thing became quiet, and the ſtratum of ſcoria prevented the oxidation. At length, after three hours longer, making ſeven in all, during which the melted maſs had frequently been ſtirred, it ſeemed to thicken; perceiving too, that it diminiſhed conſiderably in quantity, the fire was damped, and the matter left to cool in the furnace. It was afterward weighed, and found to have loſt much of its weight. Its extraordinary fracture gave reaſon to preſume a high degree of oxidation; for inſtead of being gray and granulous, it was compact, and of a ſilver white. It was interſperſed with a large quantity of ſpherical cavities, greater or leſs in ſize, which evidently announced the exiſtence of an aeriform fluid, that had been extricated during the fuſion.

The ſcoria melted.

It was blackiſh brown, of a vitreous fracture, and not to be removed.

Stirring produced extreme heat, and ſcintillating combuſtion.

The iron had loſt much in weight.

Its fracture compact and ſilvery.

Porous.

This maſs was too ſmall to be refined. Having examined the quantity of oxigen it contained in the ſame manner as with the other ſpecimens, I found that four ounces yielded 87 cubic inches of oxigen gas, and conſequently nine inches leſs than that which had been treated by means of aqueous vapour. Thus probably here too the oxidation was too powerful, and the iron was ſuper-refined. As the metal did not become doughy in the courſe of the proceſs, it muſt have been ſuperſaturated with oxigen without paſſing through the ſtate of malleable iron. The carburet, it is true, muſt have been totally deſtroyed during the operation, which produced the ſilvery hue.

There was much loſs, and the remainder acquired much oxigen,

without paſſing through the malleable ſtate.

The carburet deſtroyed.

Remarks.

VIII.

Remarks on the burſting of two Muſquet Barrels by a Charge of Gun-powder confined by Sand. W. N.

A thin muſquet barrel burſt.

PART of the barrel of a muſquet of which the internal diameter was ſix and a half tenths of an inch, was corked at one end, and fine ſand to the depth of twelve inches was poured in: upon this was poured two inches of gunpowder and a ſmall tube (of glaſs) was then ſtuck in the gunpowder, and the bore of the tube, which was about one twentieth of an inch diameter, was filled alſo with gunpowder. The length of the tube was ſufficient to reach clearly above the top of the gun-barrel, and all the reſt of the ſpace in the barrel, being about thirteen inches above the charge, was filled with ſand lightly poured in. In this ſtate the barrel was ſet up in one corner of a furnace chimney, and a match ſtuck into the glaſs tube and lighted, afforded ſufficient time for the aſſiſtants to remove out of the direct line of exploſion before the effect could take place.

The diſcharge tore the barrel into ſeveral contorted pieces in the part near the charge; the upper part fell unaltered, and its contents of ſand ran out: the lower part alſo fell down, but neither its ſand nor cork were diſturbed, nor was that portion of the barrel affected.

As the thickneſs of the iron did not exceed one thirtieth of an inch, I was deſirous of repeating the experiment with a ſtronger piece.

A thicker barrel charged with gunpowder and ſand.

A muſquet barrel, $2\frac{1}{2}$ feet long, diameter of bore five tenths of an inch, and thickneſs of metal at the breach full one quarter of an inch, was charged with 278 grains, or a little more than half an ounce troy of gunpowder, which occupied the ſpace of four inches. Upon this charge was poured fine ſand to the depth of twelve inches, weighing 1151 grains, or about $2\frac{3}{8}$ ounces troy, and upon this was lightly preſſed down a ſoft wadding of gauze paper, for the purpoſe of allowing the barrel to be placed horizontally without any ſubſequent diſturbance of its charge. It was ſafely placed in an horizontal poſition and fired at the touch hole by means of a train.

Effect of the exploſion.

The barrel was torn aſunder for the length of eight inches, the part neareſt the breach-pin being opened nearly to flatneſs.

The

The ſand remained in the barrel. Its face neareſt the blaſt was conſolidated to a very ſmall depth, and I think the maſs had been removed or elſe jammed more cloſely together; for the ſpace unoccupied between the place where the breach pin had been and the ſurface of the ſand was full nine inches. But as the ſand was not immediately noticed, I cannot be ſure that none might have been driven or fallen out, during or after the blaſt; though I am diſpoſed to think not.

It muſt be remarked, that the powder was a very full charge, and that the ſand weighed as much as ſix muſket-balls of half an inch diameter. I do not however apprehend that the barrel would have burſt with ſix balls. Remarks.

The blaſting of rocks, the ſplitting of logs of wood, and the deſtruction of artillery when on the point of being abandoned to the enemy, are the leading purpoſes in which the application of ſand to confine gunpowder is likely to become uſeful. Uſes.

IX.

Report of a Method of meaſuring the initial Velocity of Projectiles diſcharged from Fire-arms, both horizontally and with different Elevations, made to the Phyſical and Mathematical Claſs of the National Inſtitute by Mr. PRONY, *Dec.* 11, 1803. *Abridged from the Original*.*

IT is not much above ſixty years ſince experiment began to be applied with ſucceſs to the theory of projectiles. Mr. Benj. Robins, who may be eſteemed the firſt in this career, employed a pendulum to determine the initial velocity of muſket balls, meaſuring it by the arc of oſcillation. He likewiſe meaſured the recoil, by ſuſpending the gun-barrel from the pendulum. Robins firſt examined the velocity of projectiles by the pendulum, and the recoil of a gun by ſuſpending it.

About ten years after, the Chev. d'Arcy publiſhed a ſeries of experiments in the memoirs of the French Academy of Sciences, in which he employed two pendulums, againſt one of which the ball was projected, while the other, to which the gun-barrel was ſuſpended, ſerved to meaſure the recoil. D'Arcy made ſimilar experiments with two pendulums.

* *Journal des Mines*, No. 92, p. 117, May, 1804.

Fifteen

Dr. Hutton with cannon balls.

Fifteen years after this Dr. Hutton made many experiments at Woolwich with cannon balls by means of the pendulum.

Count Rumford's improvement.

About the year 1778, Count Rumford improved this method of trial, and invented a very ſimple method of ſuſpending the gun-barrel ſo that the recoil took place without the axis ceaſing to be horizontal.

Dr. Hutton's exp. are the moſt complete on the ſubject.

Laſily, Dr. Hutton reſumed the ſubject, and made a number of experiments from 1783 to 1786, with much care, and at great expenſe, on both kinds of pendulum. Theſe may be conſidered as forming the moſt complete and inſtructive treatiſe we have on experimental balliſtics.

Antoni's deſcription of Mathey's machine.

We have not mentioned the labours of Antoni, but we muſt not paſs over a machine deſcribed by him in his eſſay on gunpowder. This, which he ſays was invented by a mechanic named Mathey, conſiſts of a horizontal circle, the centre of which is ſupported by the ſuperior extremity of a vertical axis, and ſerves as a baſe to a cylindrical envelope of paper. A rotatory motion is given to this cylinder by means of a cord paſſing over a leading pulley; and the projectile being diſcharged horizontally, when the angular velocity of the machine is uniform, in a vertical plane in which the axis is found, pierces the cylinder in two points, the diſtance of the ſecond of which from the diameter paſſing through the firſt meaſures the arc deſcribed by the machine during the paſſage of the projectile.

Col. Grobert's newly invented machine deſcribed.

The machine recently invented by Col. Grobert is conſtructed as follows:

It conſiſts of two paſteboard diſks revolving ſwiftly at the extremities of an horizontal axis.

A horizontal rotatory axis about 34 dec. (11 feet) long carries at each extremity a paſteboard diſk perpendicular to it, and faſtened to it ſo that the whole may turn rapidly without deranging the reſpective poſitions of the parts.

A rotatory motion is given to the two diſks by means of a weight ſuſpended to the end of a cord, which, after having paſſed over a pulley ten or twelve yards from the ground, is rolled upon a wheel and axle level with the diſks. An endleſs chain, paſſing round the wheel and the rotatory axis of the diſks, communicates to this axis the motion which the weight in its deſcent imparts to the wheel.

The advantages this machine poſſeſſes over Mathey's conſiſt in the horizontal poſition of its axis, which admits the utmoſt degree of firmneſs and regularity in the poſition and motion of the

the diſks: in the projectile not traverſing a cylindrical ſurface, but two vertical planes, the extent and diſtance of which may be conſiderable, and thus give very accurate meaſures: and its being capable, which no other apparatus is, of meaſuring the velocities of balls of different ſizes projected at different elevations.

The projectile is fired through both diſks, and the rotation prevents the ſecond hole from being oppoſite that on the firſt paſteboard.

All that is neceſſary in uſing this apparatus is to give a uniform and known angular velocity to the diſks; and to meaſure the arc compriſed between two planes paſſing through the axis of the diſk, and one of them through the hole in one diſk, the other through the hole in its oppoſite.

Method of uſing the inſtrument.

In the trials made, the motion became ſenſibly uniform, when the weight arrived nearly in the middle of the vertical ſpace it had to traverſe, as was found by twice meaſuring the times of the third and fourth quarters of the deſcent, and afterwards comparing theſe times with the correſponding ſpaces paſſed through. An excellent ſtop-watch by Lewis Berthoud, and another by Breguet, were uſed for this purpoſe.

The deſcent of the moving weight becomes uniform.

In moſt of the experiments the vertical ſpace paſſed through by the weight was meaſured by the turns and parts of turns of the cord wound off in a given number of ſeconds, as in all reſpects moſt accurate and commodious.

The ſpace of deſcent was meaſured by turns of the cord.

To meaſure the arc a ſcreen, or paſteboard, was fixed before each diſk, a very little diſtance from it, and the hole in the firſt diſk being brought oppoſite to the hole in its correſponding ſcreen, a rod carried through the centre of theſe two holes and of the hole in the other ſcreen which would be oppoſite them, muſt pierce the ſecond diſk in the plane of the hole in the firſt; and the arc compriſed between this point and the centre of the hole in the farther diſk would meaſure the angle deſcribed by the apparatus while the ball was traverſing the length of the axis.

Method of meaſuring the arc between hole and hole.

It is obvious, that the fixed ſcreens, which give the abſolute direction of the path of the ball, afford the means of ſhewing the defect of parallelism, if there be any, between this path and the axis on which the diſks revolve.

Two fixed ſcreens are uſed to ſhew any defect of parallelism between the path of the ball and the axis.

The gun-barrel was fixed horizontally, parallel to the axis of the diſks, and at ſuch a diſtance, that the concuſſion of the air by the exploſion could not affect the motion of the neareſt diſk.

The time ſuppoſed too ſhort to allow a meaſurable arc.

One thing may naturally ſuggeſt itſelf, which is, that the time of the ball's paſſing from one diſk to the other, through a ſpace of three or four yards, muſt be leſs than $\frac{1}{100}$ of a ſecond; and it is difficult to conceive, that in ſo ſhort a ſpace the diſk could deſcribe an arc capable of being meaſured.

But it did not prove ſo.

But this difficulty is eaſily ſolved by the fact. When the motion became uniform, the wheel and axle commonly made 0·833 of a turn in a ſecond; and every turn of the wheel produced 7·875 turns of the axis of the diſks, which conſequently made 6·56 turns in a ſecond. Thus a point on the diſk three feet from the axis would move about 41 yards in a ſecond, and in $\frac{1}{100}$ of a ſecond $\frac{41}{100}$ of a yard, or nearly 15 inches, a length more than ſufficient for the moſt accurate meaſurement.

The fire-arms experimented with.

The experiments were made with a ſoldier's firelock and a horſeman's carbine, the lengths of which in the bore were 3 f. 8 in. and 2 f. 5 in. The balls were accurately weighed, found to be on a medium 382 grains troy, and each was impelled by half its weight of powder.

Formula for calculating the velocity.

The following formula was employed for calculating the velocity of the balls. Putting π for the ſemiperiphery, when radius is unity $= 3.141$; k for the ratio between the turns made by the wheel and axle and the arbor of the diſks; t the time employed by the wheel and axle to make a number of turns n; r the diſtance of the hole in the ſecond diſk from the centre; a the arc deſcribed by this hole while the ball paſſes from one diſk to the other; b the diſtance between the diſks; and V the velocity of the ball: we ſhall have the equation

$$V = \frac{2\pi n}{k t} \cdot \frac{r}{a} b.$$

Mean velocity with a carbine, 1269 f. per ſec. with a muſket, 1397.

The mean velocity deduced from ten experiments with the carbine was 1269 feet and a half in a ſecond; that from the experiments with the muſket, 1397 feet. Theſe being in the ratio of 11 to 10 nearly, it would appear, that the length of the ſoldiers's firelock might be reduced without much diminiſhing its range *: but there are other circumſtances in a military view, by which the length of the weapon uſed by the infantry requires to be regulated.

* The differences of the range are much leſs than thoſe of the velocity. *See Dr. Hutton.*—T.

The

The commiſſioners made ſome experiments with half charges or with powder only to the quantity of one fourth of the weight of the ball. In theſe the mean velocities were, for the firelock 829 feet, for the carbine 822½. Theſe velocities do not differ ſo much from each other, and conſiderably exceed the half of thoſe given by the full charge, which may be aſcribed chiefly perhaps to the more complete firing of the powder.

With ¼ of powder they were 822½ and 829.

The commiſſioners were deſirous likewiſe of making ſome experiments on the reſiſtance of the air to the motion of the ball, the diameter of which was from 15 to 16 millemetres (5·8755 lines to 6·2672.) For this purpoſe the mouth of the gun-barrel, which at firſt was 7 f. 9 in. from the neareſt fixed ſcreen, was removed to the diſtance of 67 f. 9 in. In this ſituation the mean velocity of the muſket-ball was 1127 f. inſtead of 1397, ſo that it was diminiſhed nearly in the ratio of 42 to 34. The experiments of this kind however were few in number.

The reſiſtance of the air in 20 yards diminiſhed the velocity nearly one-fifth.

There is no doubt but the dimenſions of Col. Grobert's apparatus may be enlarged, ſo as to adapt it to experiments with cannon balls; though it is not eaſy to ſay without trial what dimenſions would be compatible with accuracy of experiment.

The apparatus might be enlarged with advantage.

The Colonel likewiſe propoſes an alteration in it, for meaſuring the velocity of projectiles at different elevations, as far as 45°. The following is his contrivance for this purpoſe. Each of the diſks has a ſeparate axis. The wheel and axle has a wheel at each end, with an endleſs chain, one turning the arbor of one diſk, the other that of the other. Thus the rotatory motion imparted by the deſcending weight is communicated equally to both diſks at the ſame time, the wheels and arbors being made exactly of correſponding dimenſions. The ſtand of the diſk fartheſt from the gun is moveable in a vertical direction, ſo that it may be raiſed to the neceſſary elevation; a few links being added to the endleſs chain for every different height. As the diſk is raiſed indeed, it becomes inclined to the path of the ball; but as the greateſt diminution that can take place in this way is in the ratio of about 7 to 5, a ſufficient field is ſtill left for pointing with preciſion.

Mode of adapting it to different elevations.

To

Additional machinery for counting time, &c.

To prevent any miſtake from want of attention in the perſons employed, Col. Grobert has added certain pieces of mechaniſm to his apparatus, by means of which the weight, when it has deſcended to a certain point, puts in motion a ſecond pendulum to count the time, and a ſyſtem of wheels and pinions connected with the wheel and axle to indicate the number of turns made by it. By ſimilar contrivances it diſcharges the gun, and ſtops the pendulum and the counter of the turns at the proper time. Theſe may occaſionally be of uſe, but complicated machinery is always liable to get out of order, and it may be diſpenſed with here, if the obſervers be ever ſo little expert and attentive.

The motion of the diſks does not affect the path of the ball.

It might be ſuſpected, that the motion of the firſt diſk would cauſe ſome deflection of the ball from its true path before it reached the ſecond. To aſcertain this, three ſcreens were fixed at equal diſtances, the ſecond and third being placed before the firſt and ſecond diſks reſpectively. Now it is obvious, that the hole in the third ſcreen would not be in the ſame vertical plane with thoſe made in the firſt and ſecond, if any deviation took place.

Experimental proof.

A ball being fired through the apparatus thus arranged, a plumb line was ſuſpended before the centre of the hole in the firſt ſcreen, and the moſt accurate obſervation could not diſcover any deviation, but that the ſame line cut the centres of all the three holes. This experiment was ſeveral times repeated with the ſame event.

This owing to the velocity.

The fact no doubt is, that the extreme ſhortneſs of the time, (for the ſemidiameter of the ball is not the forty thouſandth part of a ſecond paſſing the diſk) does not allow the diſk ſenſibly to affect the path of the ball; much leſs can the ball have any effect on the motion of the diſk.

It may not be amiſs to obſerve, that the diſtance of the fartheſt ſcreen being about twelve yards only, the inflexions obſerved by Robins in diſtances of a hundred yards were not likely to take place.

Fact

X.

Fact concerning the inviſible Emiſſion of Steam into the Air.

W. N.

SOON after Mr. Giddy had mentioned to me the very remarkable and curious facts of which an account is given at page 1 of the preſent Number, I was engaged in the experiments on the ſimmering of water related at p. 216 of Vol. X. I then made an experiment which may perhaps in a ſmall degree elucidate thoſe phenomena. A ſmall glaſs tube was ſtuck through a cork, and this was then preſſed into the neck of the retort in which water was boiling over the lamp. The ſteam was emitted through this ſmaller aperture in a viſible jet of upwards of a foot in length. But when a candle was held with its flame immediately beneath the end of the tube, the jet became perfectly inviſible. To determine whether the water might be decompoſed, or the ſteam ſimply expanded ſo far as to be abſorbed by the air, or if condenſed to form a vapour too thin to be perceived, I ſuffered the hot inviſible current which had paſſed through the candle, to paſs through a larger glaſs tube. In this caſe viſible ſteam iſſued plentifully from the farther end: Hence, I am diſpoſed to judge that the large tube having kept the very hot ſteam together and cooled it ſo as to render it viſible again, there was little if any decompoſition of the water. But at the ſame time, when we conſider the diſappearance of the denſe ſmoke in Mr. Giddy's experiment, there ſeems to be great reaſon to think that the charcoal was oxigenated and gazified. If ſo, the products muſt have been expanded and inviſible ſteam, hidrogen and carbonic acid. By collecting the products in an experiment of this kind, theſe conjectures will either be verified or refuted. If the former, we ſhall have the decompoſition of water and oxigenation of carbon at a lower temperature than has hitherto been ſhewn or expected.

Steam was viſibly emitted in a current:

It became inviſible when it paſſed above the flame of a candle;

but the ſteam was not decompoſed:

Perhaps ſome part may have been changed.

Experiments

XI.

Experiments made with the Water blowing Machines of the Iron Works of Poullaouen; by Citizens BEAUNIER *and* GALLOIS, *Mine-Engineers* *.

The experiments made to ſhew the effects of a blowing machine.

OUR object was to aſcertain the differences in denſity of the air within a blowing machine, under the various circumſtances by which it might be affected; and at the ſame time, we endeavoured to find what may be the moſt advantageous mode of conſtructing the machine, to produce the greateſt effect with the leaſt expenditure of water.

Former accounts are obſcure.

One of the chief cauſes of the doubts that have ariſen reſpecting the ſuppreſſion or retaining of certain arrangements in the conſtruction, was the omitting to deſcribe the machines, the experiments with which have been compared. We ſhall therefore previouſly notice the principal diſtinctions that may be made between theſe machines, from the manner in which their effect is produced.

Two kinds of water blowing engines, as the air is received at top, or from the ſide.

Dr. Lewis obſerves, that there are two general methods of cauſing the air to be conveyed by the water in the blowing machines. In the firſt, the water receives the air by the ſummit of the machine; in the ſecond it receives it by lateral apertures: and he lays it down as a principle, that thoſe circumſtances, which promote the effect in the one caſe, are detrimental to it in the other.

General obſervations of Dr. Lewis. The engine is an upright pipe through which a ſhower of water and air deſcended.

He obſerved further, that if the water be at reſt in the funnel of the machine, (ſee Plate III. Fig. 2.) and afterward have liberty to run off, it carries little or no air with it; that if the water have a gyratory motion in the funnel, it carries down a conſiderable quantity; and that if it fall from a certain height, ſo as to have been greatly divided, it carries ſtill more: that if the water flow through a pipe with lateral apertures, it receives air through theſe apertures, even when its motion is ſlow: that if the pipe be of equal diameter throughout, the quantity of air thus received is inconſiderable; but if the diameter be diminiſhed to a certain degree at the part where the apertures are made, the quantity of air is greater than could have been introduced through the funnel

Lateral apertures preferable for admitting the air.

* Tranſlated from the Journal des Mines, No. 91.

without

without any lateral openings to the air: laſtly he obſerves, that air conveyed downward from the top of the tube, or the funnel, prevents the introduction of the freſh air by the lateral apertures, which in this caſe, inſtead of receiving more air, let that which has been already introduced eſcape.

Lewis concludes, that the two methods by which air may be made to deſcend with a ſtream of water, ought not to be united in one machine; and that the machine conſtructed with a pipe, a funnel, and apertures to let the air enter around or below the throat, produces the moſt powerful effect.

The machine on which we made our experiments was of the conſtruction which Dr. Lewis has deemed moſt advantageous. See Fig 2. The machine of Poullaouen deſcribed.

The height of the fall, taken from the bottom of the channel that conveys the water to the upper part of the barrel B, is 21 feet 6 inches. Height of the fall.

The height of the funnel, from the bottom of the ſame channel to the throat *x*, is ſeven feet. This funnel is of the ſhape of a fruſtum of an inverted cone, the larger diameter of which is 12 inches, the ſmaller four. The remainder of the tube down to the barrel, is a cylinder eight inches in diameter. Funnel at top of the pipe.

The plank N, 12 or 13 inches wide, is fixed one foot below the head of the barrel. The barrel is ſix feet high. Barrel or air veſſel.

The water iſſues out of the barrel by the triangular apertures *t t t*, and is conveyed away to a drain by the channel M, the bottom of which is four feet higher than that of the barrel. The water flows off beneath;

The air compreſſed by the external water, the level of which, as will ſoon be proved, is from 27 to 30 inches above that of the water in the barrel, eſcapes through the tube P, which is a hollow cylinder five inches in diameter. and the air is conveyed through a pipe at top.

This tube P, called alſo the air-pipe, terminates in a conical nozzle, having an aperture of two inches only. Air-holes in the upright pipe.

Immediately below the throat *x*, are four air-holes *y y*.

This being premiſed, we proceed to the inſtrument employed by us for determining the denſity of the air in the machines.

It was invented by Citizen Vergnies Bouiſchére, proprietor of the iron works at Vic-Deſſos, in the ci-devant county Gage for meaſuring the denſity of the air.

county of Foix, and is a particular kind of barometer, to which the name of water anemometer has been given. See Fig. 6.

It is a ſhort barometer gage inſerted, the fluid being water.

It is compoſed, 1ſt. of a cylinder A; 2d. of a tube *c*, bent twice, the lower extremity of which is ſlightly conical, and terminates about two inches below the bottom of the cylinder; 3d. of a graduated tube *d* inſerted in a vertical poſition into the cylinder, and reaching below the level *n* of the water contained in it.

The tube *c* being inſerted into an auger hole made in the ſide of the blowing machine, and ſtopping that hole cloſely, the internal compreſſed air communicates with the water contained in the anemometer, preſſes upon it, and in proportion to its denſity raiſes to a leſs or greater height in the graduated tube.

The cylinder A and the tube *c* are of tin. The lower part of the tube *d* to the height of nine inches is of the ſame material, to which is fitted a glaſs tube about 36 inches long.

The cylinder A is four inches high and as many in diameter. The greateſt diameter of the curved tube is half an inch, the ſmalleſt, at the extremity, a third of an inch. On obſerving however, that the ſize of this opening contributed to increaſe the extent of the oſcillations in the graduated tube, we endeavoured to diminiſh it as much as poſſible. For this purpoſe we cloſed the lower part of the tube *c* with ſealing wax, in which we afterward made a very ſmall aperture by paſſing a heated needle through it.

The tube *d* was divided by a ſcale of inches, beginning from the ſurface of the water contained in the cylinder *.

ACCOUNT OF THE EXPERIMENTS.

Experiments with the blowing machine.

I. *Experiments relative to the Expenditure of Water, and the Quantity of Air diſengaged.*

The blowing machine No. 1, ſee the horizontal projection, Fig. 1. to which for the ſake of clearneſs we ſhall refer our different experiments, ſerved for the trial. It was placed in a T, oppoſite the machine No. 2, deſtined for the ſame pur-

* The great difference between the diameter of the tube *d*, and that of the cylinder A allows the level *n* to be conſidered as conſtant.

poſe.

poſe. The afflux of water into each was regulated by means of the hatches *a* and *b*, and the diſtant floodgate Q, placed in the principal channel D. See the plan Fig. 1. Plate .

The anemometer was placed in *o*, Fig. 2, in the direction of the vertical tube P protracted. The hatch placed in *b* was let down, ſo as to prevent the paſſage of the water that way. The hatch placed in *a* was raiſed, and the flow of water regulated by means of the floodgate Q. This flow we varied, till we found the water in the graduated tube raiſed as high as could be effected without any other change of circumſtance. When we were certain we had attained this point, and that no variation in the quantity of water flowing off took place, we made the following obſervations.

1. The mean depth of the water in C, in the little channel, juſt before the T, was 15 inches 6 lines. Obſervations.

2. The mean depth of the water in the great channel, was 18 inches 9 lines.

3. The water roſe to 26 inches in the graduated tube. The oſcillations varied between 25 and 27 inches, but ſeldom reached the latter height.

4. The velocity of the water in the great channel having been obſerved, the following data were obtained.

Examined by means of ſimple floaters of paper, on an extent of 24 feet, we had,

1ſt. The ſpace paſſed through in two minutes = 61 feet, 8 inches, 6 lines.

2d. The ſpace paſſed through in four minutes = 120 feet, 6 inches.

The ſame velocity examined with cork floats, ſupporting little balls of wax, the weight of which was augmented by bits of lead, ſo that they ſwam in the middle of the ſtream with a gravity little exceeding that of water, we had for a mean of the ſpace paſſed in two minutes, 63 feet, 7 inches, 4 lines. Method of meaſuring the velocity of the water.

If we compare theſe different reſults, we ſhall find, that the mean velocity of the water may be eſtimated at 30 feet, 11 inches, 1 line, a minute: but as it appears to us, that the reſults afforded by the cork floats muſt approach neareſt the truth, we will pay no regard to the quantities before obtained, and eſtimate the mean velocity of the water in the greater channel, at 31 feet, 9 inches, 8 lines, a minute.

Conſumption of water.

Now the breadth of the channel employed is 3 feet, inches, and we obſerved, that the current, the velocity of which we have given, is 18 inches 9 lines deep. Hence we may conclude, that the conſumption of water by the machine, under the circumſtances above mentioned, is 173 cubic feet in a minute, the height of the column of water in the inſtrument being 26 inches.

Quantity of air emitted.

From the method deſcribed in the Hydrodynamics of Boſſut, we calculated the quantity of air which this maſs of water cauſes to iſſue from the machine in a given time. This quantity of air was found to be 7.35 cubic feet in a ſecond, or 441 in minute *.

II. *Experiments on the Effect of the Air-Holes.*

Effect of the air-holes aſcertained by experiment.

1. On ſtopping the four air holes, the water in the tube of the inſtrument deſcended to nine inches, and oſcillated very little. The effiux of the water from the machine acquired a velocity ſufficient to diminiſh the depth of the water in the little channel C, Fig. 1 and 2, near the T, ſix inches.

2. One of the air-holes being opened, the water in the tube oſcillated between 22 and 24 inches. The mean = 23 inches.

3. A ſecond air-hole being opened, the mean height of the water in the tube was 25 inches.

4. A third air-hole being opened, the columns of water in the tube roſe to its former height of 26 inches.

5. The fourth hole being opened, no perceptible alteration in the inſtrument took place, which proves, that this hole has no effect on the machine.

III. *Experiments on the Uſe of Croſſes placed at the ſuperior Orifice of the Machine.*

ther croſs bars in the top of the tube be advantageous.

Some iron-maſters are accuſtomed to place two round bars in the form of a croſs at the upper orifice of the funnel of the machine. Theſe they imagine increaſe the effect of the machine by dividing the water at the moment of its fall.

Cylindrical bellows of Namur give more air with leſs water.

* If theſe reſults be compared with thoſe of the cylindrical bellows of the country of Namur, deſcribed by Cit. Bailbet, in No. 16, of the Journal des Mines, it will appear, that, to give out an equal quantity of air, the quantity of water expended by the blowing machines, with a fall more than twice its height, is nearly double that employed to move the cylindrical bellows.

To

To judge of this in the caſe before us, we fitted in one of theſe croſſes, all the other circumſtances remaining as above, and then obſerved the progreſs of the inſtrument.

The column of water in the tube frequently deſcended to 24 inches, and ſeldom roſe to 26: whence we may eſtimate the mean height, which before was 26 inches, only $24\frac{3}{4}$. They diminiſh the effect.

Now this difference occaſions a diminution of velocity in the efflux of the air, and conſequently ſhews the faultineſs of this method under the circumſtances here mentioned.

IV. *Experiments on the Effect of Hatches placed near the Orifice of the Machine.*

The hatch *a* Fig. 1. was replaced in the grooves adapted to the channel. We altered its height from the bottom of the channel, obſerving the movements of the anemometer, in order to find the poſition moſt favourable for the effect of the machine. Advantage of regulating the influx of water.

The mean height of the column of water in the tube never exceeded 28 inches, the elevation of the lower part of the hatch above the bottom of the channel, being then five inches one line; and it is remarkable, that the difference of a ſingle line in this elevation lowered the water in the tube conſiderably.

V. *Experiments on the Croſſes when the Hatch is uſed.*

The hatch being placed as has juſt been ſaid, we fitted the croſs again at the ſuperior aperture of the funnel, when the water in the tube of the anemometer ſunk. We then varied the height of the hatch above the bottom of the channel, obſerving the progreſs of the inſtrument, to determine the moſt advantageous poſition for it under the preſent circumſtances. When thus regulated.

The elevation of five inches eight lines was now found the moſt favourable to the effect. With this the water oſcillated in the tube between 28 and 30 inches, moſt frequently reaching the latter height, which we could never bring it to exceed, whatever changes we made in the arrangement of the parts that compoſe the machine. The croſs produced more air,

If we compare the ſituation of the hatch before the addition of the croſs, with that which is moſt ſuitable in the caſe before us, we find an increaſe of ſeven lines in the height but expended more water.

from the bottom of the channel: now this addition to the height conſiderably increaſes the quantity of water expended by the machine.

Concluſions from theſe Experiments.

General concluſions. The engine by 173 cubic feet falling through 21 feet, drove out 441 cubic feet of air in a minute, under a preſſure of 26 inches of water on nearly two inches of mercury: which is not quite one pound per ſquare inch.

(A.) Under the circumſtances related in the firſt ſet of experiments.

1. The expenditure of water for the blowing machine with which they were made was 173 cubic feet in a minute.

2. The air emitted from the aperture of the nozzle, being two inches in diameter, when the anemometer was at 26 inches, was 441 cubic feet in a minute.

(B.) Of the four air-holes in the machine, three only contribute to the effect produced.

(C.) The hatch placed near the orifice of the machine increaſed its effect, when the lower part of it was raiſed five inches one line above the bottom of the channel to which it was fitted.

(D.) A croſs placed at the upper orifice of the machine diminiſhes its effect when the hatch is taken away: on the contrary they increaſe it, if the hatch be ſo placed, as to be five inches eight lines above the bottom of the channel, an elevation greater than that mentioned in the preceding paragraph (C.) and which increaſes the expenditure of water.

From theſe reſults it may be inferred, that the croſs ſhould not be uſed in ſeveral caſes, where the quantity of water with which the machine is ſupplied, is confined within certain limits.

ANNOTATION. W. N.

The water blowing engine farther explained.

The blowing engine deſcribed in the preceding memoir acts upon the principle of the lateral adheſion of fluids, upon which Venturi has ſo ably written, in a treatiſe given entire in our Quarto Series of this Journal, and ſeparately publiſhed afterwards by Taylor in Holborn. The ſhower of water in its deſcent through the vertical pipe carries down a maſs of air along with it, in the ſame manner as a ſhower of rain on the flat ſurface of the ſea produces that temporary blaſt of wind,

which ſeamen term a ſqwall, and is ſufficiently violent to carry away the maſts of a ſhip, if the ſails be not reduced in time.

It is evident that this engine poſſeſſes the deſireable qualities of cheapneſs and ſimplicity; and Lewis who has written ſomewhat fully upon it, in his Philoſophical Commerce of Arts, from experiments of his own, aſſerts, that it requires much leſs water for working it than any other kind of bellows in uſe. I have no doubt but that many occaſions muſt offer in which it would be beneficial; but whether its expence of water be comparatively ſmall, and its power in any caſe equal to the ſupply of our ſmelting furnaces, may be deſerving of more enquiry.

Numerical ſtatement by Mr. Roebuck, of the effect of a ſteam engine in affording a blaſt of air,

In the excellent paper of Mr. Roebuck on the Devon iron works, inſerted in the fifth Volume of the Edinburgh Tranſactions, and alſo in the Quarto Series of this Journal, there are ſome numerical facts reſpecting the blaſt afforded to iron furnaces by iron cylinder bellows worked by the ſteam engine; and as they agree very well with others given in my Chemical Dictionary, under the article *Trompe*, I will ſtate them in this place. Mr. Roebuck affirms, that one iron furnace was excited by a blowing cylinder, which gave 155 cubic feet of air 16 times per minute, which numbers give a product of 2480 cubic feet. This is $5\frac{1}{2}$ times the quantity emitted by the blowing engine in the text. The ſteam engine was eſtimated to act by a preſſure of 13062 lbs. anſwering to $2\frac{3}{4}$ lbs. on the ſquare inch of the air piſton, and this multiplied by four feet eight inches, the length of ſtroke, and by 16, the number of ſtrokes, gives 975296 for the weight multiplied by its fall in feet.

compared with the water blowing engine.

Now the machine in the text was worked by 173 cubic feet, or 10812 lbs. of water falling through 21 feet, which gives a product of 227052, or more than one fourth of the firſt mover of the ſteam engine blaſt, inſtead of one fifth and a half. The blowing engine therefore conſumed more water by one fourth than would have been required to produce its effect, according to what was done by the ſteam engine. But the ſteam engine drove out its air under a reaction of between five and ſix inches of mercury in the gage; a velocity which being more than Mr. Roebuck found neceſſary, was a diſadvantageous waſte of power. The velocity of the water blowing engine produced by its preſſure of two inches, is moſt probably too ſmall; and if ſo, the multiplication of

The ſteam engine has more power.

theſe

theſe engines would not be adviſeable, even if Lewis had been in the right in ſuppoſing them to ſave water. Theſe rough computations, or rather eſtimates, are ſufficiently near for data ſo looſe as thoſe upon which we have operated; and they appear to ſhew that the principal, and perhaps the only recommendation of the water engines is, that many of them may be made and applied at a ſmall charge, in ſituations where water with a proper fall is plentifully to be had.

XII.

A Method of rendering the long and ſhort Vibrations of a Balance, governed by a ſpiral Spring, preciſely equal in Duration. By Mr. CHARLES YOUNG. *In a Letter from the Inventor.*

To Mr. NICHOLSON.

SIR,

No explanation has been given why the long and ſhort vibrations of a balance are different.

I HAVE lately tried many experiments upon ſprings, with a view to obtain ſome knowledge of the cauſes which govern an effect that is very troubleſome to all makers of chronometers; namely, that the vibrations of the balance through ſhort arcs, conſiſting of perhaps ninety degrees, are in ſome inſtruments performed in longer, and in others in ſhorter times than thoſe long arcs, ſuch as of four hundred. It is certain that no ſatisfactory reaſons have been given, either in England or in France, to ſhow how this irregularity is produced.

A balance ſuſpended by a ſtrait wire had its long and ſhort vibrations equal.

I made a piece of braſs to ſerve as a large watch balance, and ſuſpended it by a bit of ſpring wire, on which it could vibrate as an axis, then having turned it four or five times, I left it to regain its natural poſition*. It performed all its

* This method of ſuſpenſion has been uſed for philoſophical purpoſes, by Mr. Mitchell, (ſee Prieſtley's Optics,) by Mr. Cavendiſh, (ſee Philoſ. Tranſ. and alſo this Journal quarto II. 446.) by Mr. Coulomb, in his numerous experiments on Electricity and Magnetiſm; and by Mr. Berthoud, in his Time Piece, No. 24. See his Treatiſe de la Meaſure du Temps, p. 50. It does not appear that this ſpring has yet been uſed by itſelf in time pieces. N.

oſcillations

oſcillations preciſely in equal times, whatever was their extent, whether they conſiſted of thirty degrees, or of three thouſand. It therefore returned to the place at which it was at reſt with velocity exactly proportioned to the forces employed to remove it. From this experiment I concluded, that the balance ſpring of a watch is not in a ſituation to exert this natural quality, but that the diſtortion into which it is thrown, is ſuch as deſtroy this valuable property of iſochroniſm.

The principal circumſtance by which the ſpiral balance ſpring appears to me to be cramped, and prevented from operating by its natural action throughout, is, that the outer extremity is fixed by the ſtud, ſo that it cannot expand and contract in its coils every where alike, as it ought to do. To remedy this, I attached the ſtud to a ſtraight ſpring, lying in the direction of the tangent of the ſpiral, continued from that extremity. This ſpring by its eaſy action allows the ſpiral to approach the centre, and retire from it with great regularity; and, what is moſt material, it can with certainty be reduced to ſuch a ſtrength, that the long and ſhort vibrations of the balance will prove perfectly equal when this adjuſtment is made. For upon the ſtrength of this ſhort ſpring depends the freedom with which the axis of the balance is enveloped by that ſpring which regulates its motion. The ſpring ſtud affords a good banking; for the banking pin on the balance may be eaſily placed ſo as to ſtrike upon the end of the ſtud in the caſe of extreme vibration.

The ſpiral ſpring is cramped by the ſtud.

but when its outer end is made free by a ſtraight ſpring its vibrations are all iſochronal.

I am, Sir,

Your's moſt reſpectfully,

CHARLES YOUNG.

Wood Street, Aug. 23, 1805.

SCIENTIFIC

SCIENTIFIC NEWS.

Compoſition of Muriatic Acid.

Letter on the compoſition of muriatic acid.

IN the third number of the Edinburgh Medical and Surgical Journal, publiſhed July 1 laſt, is the tranſlation of a letter forwarded to the editors of that work by the celebrated Fabbroni. It bears date from Piſa, May 9, 1805, and is written by Dr. Franciſco Pacchioni, profeſſor of natural philoſopy in the univerſity of that city, to Sig. Lorenzo Pignotti.

Water decompoſed by galvaniſm ſhewed

After ſome prefatory obſervations, the writer announces that he has ſucceeded by galvaniſm in obtaining ſatisfactory evidence of the nature of the conſtituent principles of muriatic acid. He expreſſes his confidence that the ſimplicity of his apparatus and means have ſecured him againſt illuſion; but for want of time he forbears to relate the whole ſeries of his experiments. His reſults are,

—that muriatic acid is an oxide of hidrogen.

1. Muriatic acid is an oxide of hidrogen. 2. In the oxigenated muriatic acid and therefore, *á fortiori*, in muriatic acid there is a much leſs proportion of oxigen than in water. 3. Hidrogen may have very many and different degrees of oxidation.

Some account of the experiment.

The author informs us that having, by accurate experiments, aſcertained the true theory of galvaniſm, he readily diſcovered a very ſimple and exact apparatus, in which he could diſtinctly perceive the changes which happen to water, which, from the continued action of the galvanic pile, is conſtantly loſing its oxigen at the ſurface of a wire of very pure gold immerſed in it.

Water deprived of oxigen firſt became acid;

With this apparatus, which I conjecture muſt have been the ſame as that of Davy, in which the oxigen and hydrogen were given off in ſeparate veſſels of water, he obſerved that pure oxigen was emitted from the gold wire, that the water became acid, and when by proceeding in the operation until the reſidual fluid occupied about half the capacity of the receiver (that is, I preſume, when half the fluid in one of the veſſels had diſappeared) the remainder was found to be of an orange colour, more deep the leſs quantity of fluid. It reſembled a ſolution of gold. From the lower orifice of the veſſel, which was cloſed with a piece of taffetas and then with double bladder, a ſmell was emitted of oxigenated muriatic acid. The gold wire appeared corroded. The bit of taffetas

—and then appeared to have diſſolved part of the gold wire.

which

which had been in contact with the coloured fluid had undergone an action which rendered it easily to be torn. Round the edges of the vessel on the bladder there was a deep purple ring and within that a circular space rendered colourless or white. A drop of the fluid itself tinged the skin of the hand after some hours, of a beautiful rose colour.

The same liquid, possessing constantly the same qualities, was obtained in various repetitions of the experiment. It was shewn to contain a volatile acid by the white vapours which were formed by ammonia placed near it. It threw down a curdy precipitate from nitrate of silver, which the author concludes to have been the muriate of that metal; and from the whole of the facts he deduces the results first enumerated at the beginnig of this abstract, respecting the composition of muriatic acid from water by depriving it of part of its oxigen. He promises to treat of the other oxides of hydrogen in a memoir shortly to appear.

Qualities of the fluid. It contained muriatic acid.

The origin and nature of the muriatic acid being thus, as the author observes, determined, there is no longer any mystery in its formation, nor in that of the muriatic salts in the vast extent of the ocean.

Hence the origin of the salt of the sea.

The editors of the respectable Journal, from which I have made this extract take notice of the early discovery of Cruickshank (published by him in our quarto series for 1801) that infusion of litmus was reddened by one end of the pile and infusion of Brazil wood rendered purple by the other, which he ascribed to the formation of nitrous acid and ammonia; and they also quote the discovery of muriatic acid being formed by the galvanic action by Mr. Peele of Cambridge, which was announced in Mr. Tilloch's Philosophical Magazine a few days before Professor Pacchioni's letter was published at Pisa. Mr. Peele's letter bears date April 23, 1805. He took a pint of distilled water and decomposed half of it by means of galvanism; the other half, being then evaporated, left a small quantity of muriate of soda or common salt. Great attention had been paid to the purity of the water; and upon a careful repetition the same result was again had. In a postscript he mentions that a friend of his had tried the experiment and succeeded in the same manner.

Acid and alkali observed by Cruickshank in 1801 to be formed by galvanism,

—and common salt by Peele in 1805,

Or muriate of soda.

Literary

Literary and Philoſophical Society of Newcaſtle upon Tyne.

Twelfth report of the Newcaſtle Lit. and Phil. Society.

THE Literary and Philoſophical Society of Newcaſtle-upon-Tyne have publiſhed their twelfth year's report. The ſpirited union of literature, ſcience and practical reſearch continues to form the character of their proceedings. Their library encreaſes no leſs in value than in magnitude, and they have liberally reſolved "that the ſubſcribers to the public li-"brary at North Shields (and to other ſimilar inſtitutions which "ſhall afford an equal accommodation to the members of the "Newcaſtle Society) ſhall be admitted to the rooms without "introduction on producing to the librarian a certificate of "their being members of ſuch inſtitutions." I will not ſuppoſe that any of my readers will conſider this information as merely local. The advantages of provincial ſocieties of eſtimable and well informed men is of high national importance, and it cannot but be of general intereſt that ſuch enlightened proceedings as are adopted in one part of the kingdom ſhould be known and imitated in every other quarter, where ſimilar circumſtances may render them deſirable.

Blaſting rocks in Mr. Jeſſop's method,

I have much pleaſure in adding the teſtimony of Northumberland in favour of the improvement in blaſting rocks, which Mr. Jeſſop communicated laſt December, through the channel of our Journal.

——tried with ſucceſs in Northumberland.

At* the meeting in April, 1804, Mr. Fogget of Sheriff-Hill reported, that the new mode of blaſting with ſand, deſcribed in the Philoſ. Journal had been tried by him, and that, contrary to his expectation, it had anſwered every purpoſe of the old mode, with a conſiderable ſaving of powder, and of more than one-third of the labour, and with an entire freedom from riſk.

At the meeting in May, Mr. Fogget preſented a ſection of two holes drilled and prepared for blaſting according to the new method: One perpendicular, in which the charge of powder being introduced and the communication-ſtraw placed, the remainder of the hole is filled up with fine dry ſand: the other a horizontal or aſcending hole; in which the powder and ſand, being made up into a cartridge, is in the act of being thruſt up to the fartheſt extremity of the hole, by a blunt-pointed pricker put in by the ſide of the communication ſtraw.

* Report, page 5.

At

At the meeting in June an account was communicated by Mr. Thornhill of an accident having happened in Gateſhead-Park colliery, by which one man loſt his life and another had been ſeverely wounded, in conſequence of the powder having taken fire in the common mode of ſtemming, or ramming down the charge with fragments of ſtone. A caſe was alſo cited by Mr. Horn of a perſon who had lately been brought from Alſton Moor with his ſkull fractured by a ſimilar exploſion. Danger of the old method of ramming down the charge.

New Proceſs for Steeping Hemp.

THE new proceſs of M. Bralle for ſteeping hemp, which has the advantage of ſaving time, capital, and the health of numerous individuals, and of which an account was given at page 86 of our laſt volume, has been repeated in one of the provinces of France, to the entire ſatisfaction of the inhabitants, who might be ſuppoſed the leaſt inclined to deviate from their accuſtomed habits. The ſtaple was found to be excellent, and of a ſuperior ſtrength and quality when ſpun into thread, and alſo after it had paſſed the loom in the form of cloth. Steeping of hemp.

Medical Theatre, St. Bartholomew's Hoſpital.

THE following courſes of Lectures will be delivered at this theatre during the enſuing winter.

On the theory and practice of medicine, by Dr. Roberts and Dr. Powell.

On anatomy and phyſiology, by Mr. Abernethy.

On the theory and practice of ſurgery, by Mr. Abernethy.

On comparative anatomy and phyſiology, by Mr. Macartney.

On chemiſtry, by Dr. Edwards.

On the materia medica, by Dr. Powell.

Anatomical demonſtrations and practical anatomy, by Mr. Lawrence.

The anatomical lectures will begin on Tueſday, October 1ſt, at two o'clock, and the other lectures on the ſucceeding days of the ſame week.

Further particulars may be learned by applying to Mr. Nicholson, at the apothecary's ſhop, St. Bartholomew's hoſpital.

AN

Medical Inſtitution.

AN inſtitution has been lately eſtabliſhed in London for the purpoſe of promoting a liberal and uſeful intercourſe among the different branches of the medical profeſſion, and of affording a centre for the reception of communications, and for the formation of a ſelect and extenſive profeſſional library. It is called the *Medical and Chirurgical Society of London*, and it comprizes a conſiderable number of profeſſional men of the firſt character. The meetings (which will commence in October) will be held at the Society's apartments, Verulam-buildings, Gray's-Inn, where any communications, or donations of books are requeſted to be ſent, directed to the ſecretaries.

The following is a liſt of the officers and council for the preſent year.

PRESIDENT, WM. SAUNDERS, M.D. F.R.S.

John Abernethy, *Eſq.* F.R.S. *Vice-Pres.*
Charles Rochemont Aikin, *Eſq.* *Sec.*
Wm. Babington, M.D. F.R.S. *Vice-Pres.*
Matthew Baillie, M.D. F.R.S.
Thos. Bateman, M.D. F.L.S.
Gilbert Blane, M.D. F.R.S.
Sir Wm. Blizard, F.R S. *Vice-Pres.*
John Cooke, M.D. F.A.S. *Vice-Pres.*
Aſtley Cooper, *Eſq.* F.R.S. *Treas.*
James Curry, M.D. F.A.S.
Sir Walter Farquhar, *Bart.* M.D.
Thompſon Forſter, *Eſq.*
Algernon Frampton, M.D.
John Heaviſide, *Eſq.* F.R.S.
Alex. Marcet, M.D. *For. Sec.*
David Pitcairne, M.D. F.R.S.
Hen. Revell Reynolds, M.D. F.R.S.
H. Leigh Thomas, *Eſq.*
James Wilſon, *Eſq.* F.R.S.
John Yelloly, M.D. *Sec.*

Properties

Properties of blued Steel not generally known.

IN making ſprings of ſteel the metal is drawn or hammered out and faſhioned to the deſired figure. It is then hardened by ignition to a low red heat and plunging it in water, which renders it quite brittle. And laſtly, it is tempered either by blazing or blueing. The operation of blazing conſiſts in ſmearing the article with oil or fat, and then heating it till thick vapours are emitted and burn off with a blaze. I ſuppoſe this temperature to be nearly the ſame as that of boiling mercury, which is generally reckoned to be at the 600° of Fahrenheit, though, for reaſons I ſhall in future mention, I think this point requires to be examined. The operation of blueing conſiſts in firſt brightening the ſurface of the ſteel, and then expoſing it to the regulated heat of a plate of metal or a charcoal fire, or the flame of a lamp until the ſurface acquires a blue colour by oxidation. The remarkable facts which I have here to preſent to the notice of philoſophers are that Mr. Stodart aſſures me that he has found the ſpring or elaſticity of the ſteel to be greatly impaired by taking off the blue with ſand paper or otherwiſe; and, what is ſtill more ſtriking, that it may be reſtored again by the blueing proceſs without any previous hardening or other additional treatment.

Method of making ſprings.

Hardening, blazing and blueing.

A blue ſpring injured by brightening and reſtored by blueing again.

Mr. Hardy, who is meritoriouſly known as a ſkilful artiſt,* aſſured me ſome time ago that the ſaw-makers firſt harden their plates in the uſual manner, in which ſtate they are more or leſs contorted or warped, and are brittle;—that they then blaze them; which proceſs deprives them of all ſpringineſs, ſo that they may be bended and hammered quite flat, which is a delicate part of the art of ſaw making;—and that they blue them on an hot iron which renders them ſtiff and ſpringy without altering the flatneſs of their ſurface. Mr. H. finds that ſoft unhardened ſteel may be rendered more elaſtic by blueing, and that hard ſteel is more expanſible by heat than ſoft.

Saw makers harden their ſteel; then ſoften it by blazing; and render it elaſtic by blueing.

Soft ſteel blued.

Hard ſteel expands more by heat.

It is very difficult to reaſon or even to conjecture upon theſe facts. They certainly deſerve to be verified by a direct proceſs of examination, which I intend to make, and ſhall ſtate the reſults in our next number. N.

* See his banking for time pieces in our XI. Vol. page 114.

Cor-

Preſervation of Succulent Plants.

Whether the fact be generally in poſſeſſion of the collectors of plants I know not, but it will certainly be inſtructive to many readers to be informed that green ſucculent plants are much better preſerved after a momentary immerſion in boiling water than otherwiſe. This treatment, which I am told is adopted for the economical preſervation of cabbage and other plants which are dried for keeping, deſtroys the vegetable life at once, and ſeems to prevent an after proceſs of decay or mortification, by which the plant would have been more conſiderably changed, if it had not been ſo ſuddenly killed.

Corrections to the 11th Volume.

P. 159, l. 2, *area* r. *era*.—l. 21, *Agy* r. *Aqy*.—l. 36, $q \times p - 2n$ r. $q + p - 2n$.—l. 12, *for* r. *of*.—l. 28, put a comma before *that*.—p. 236, l. 20 from the bottom, after the words *preſerved as* add *as perfect as poſſible, but the preſs recommended by Dr. Withering does not appear calculated, &c.*—

Dr. Boſtock's eſſay upon animal fluids which was communicated by the author and inſerted in our laſt number, appeared alſo in the third number of the Edinburgh Medical and Surgical Journal. This by a caſual omiſſion of the friend who forwarded me manuſcript was not intimated to me until ſeveral days after the paper came to hand.

Engine for raising Water, by Mr. Wm Close.

Fig. 1.

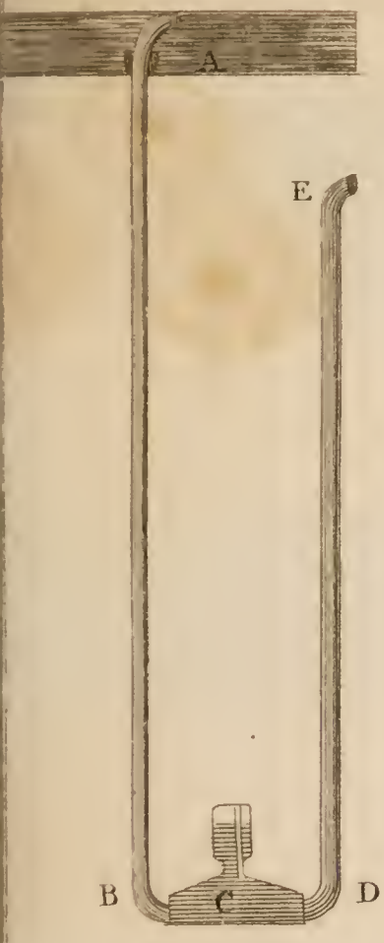


Fig. 3.

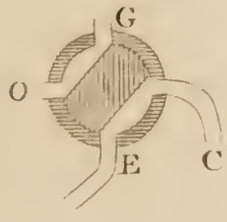


Fig. 4.

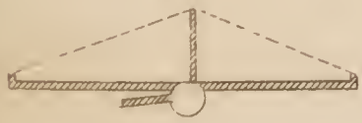
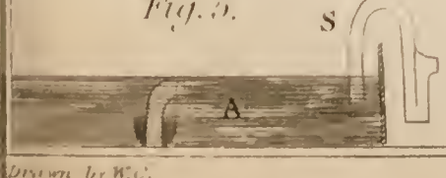


Fig. 5.



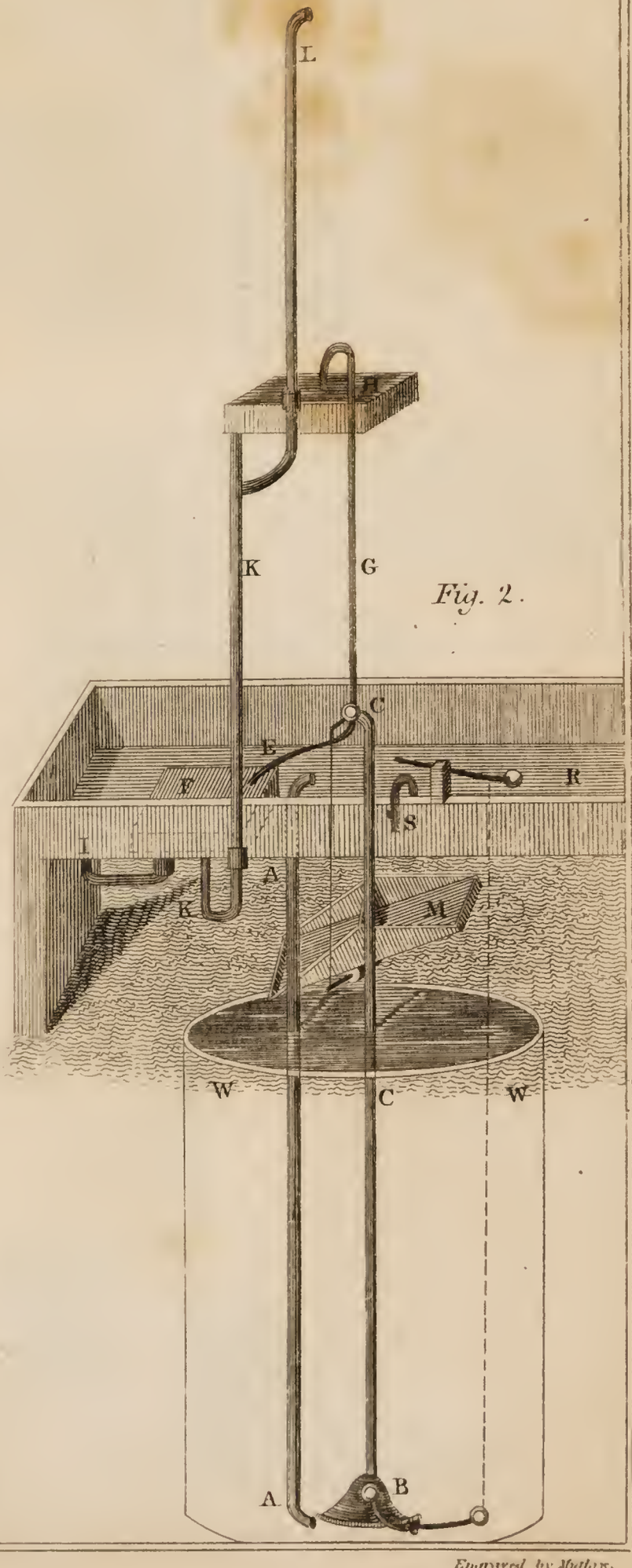


Fig. 2.

Drawn by W.C.

Engraved by Mathew.

Furnace for bending Wood.

Fig. 1.

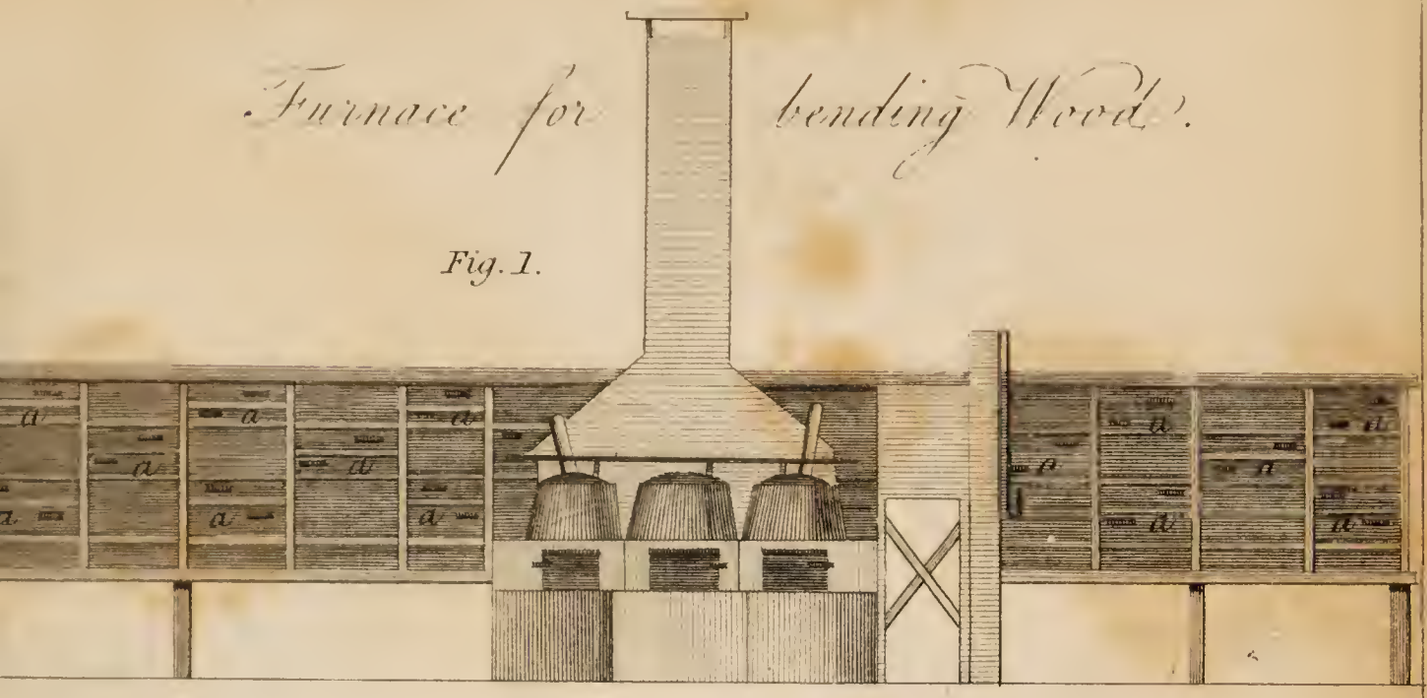


Fig. 2.

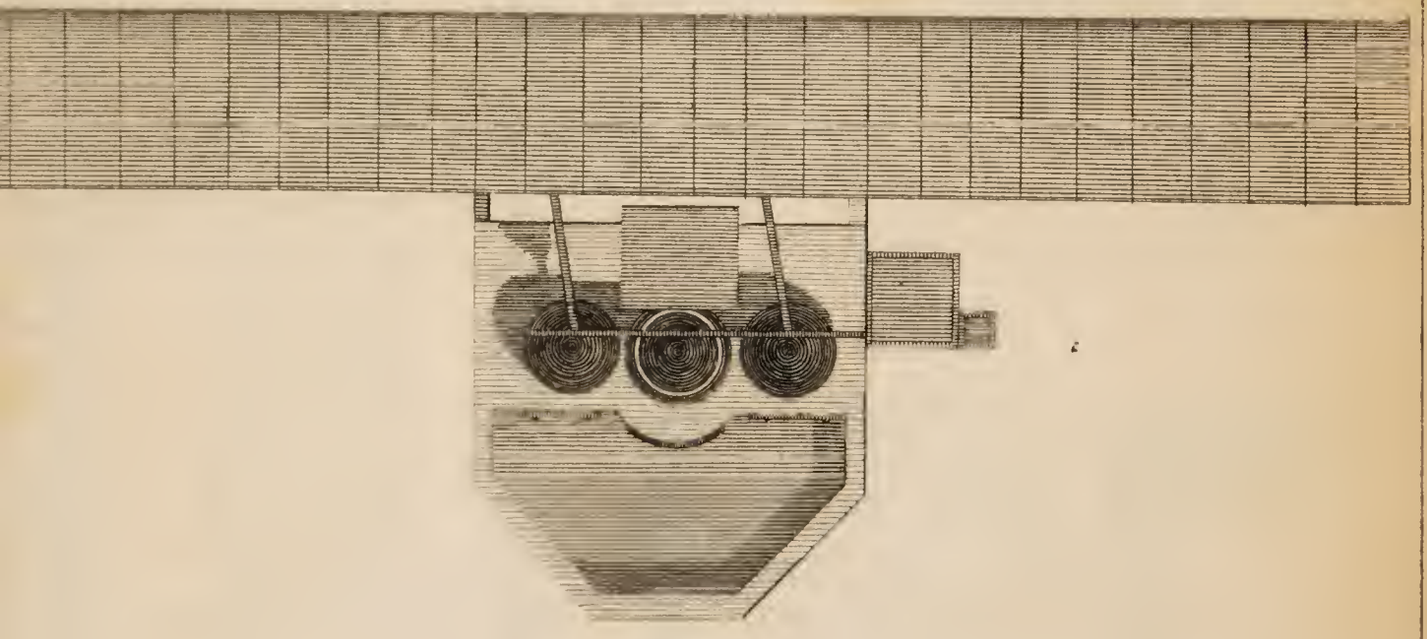
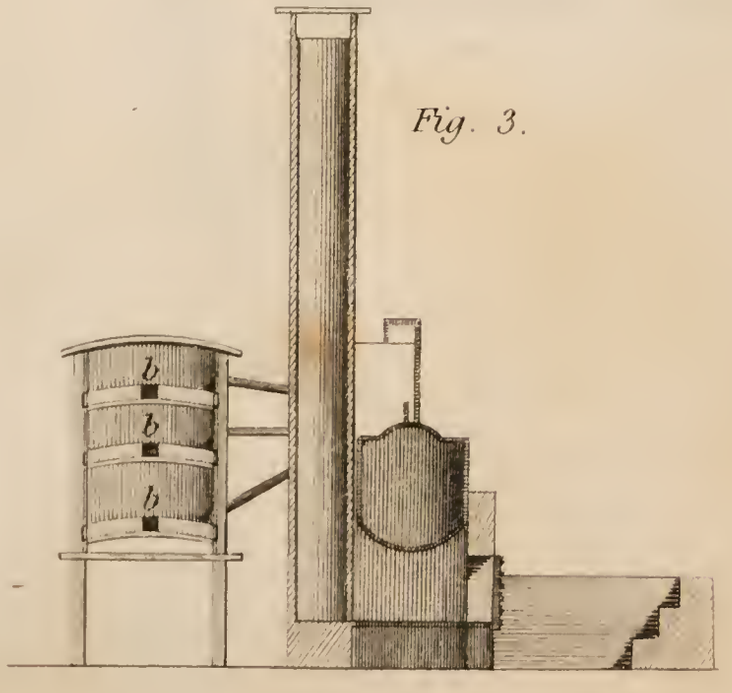


Fig. 3.

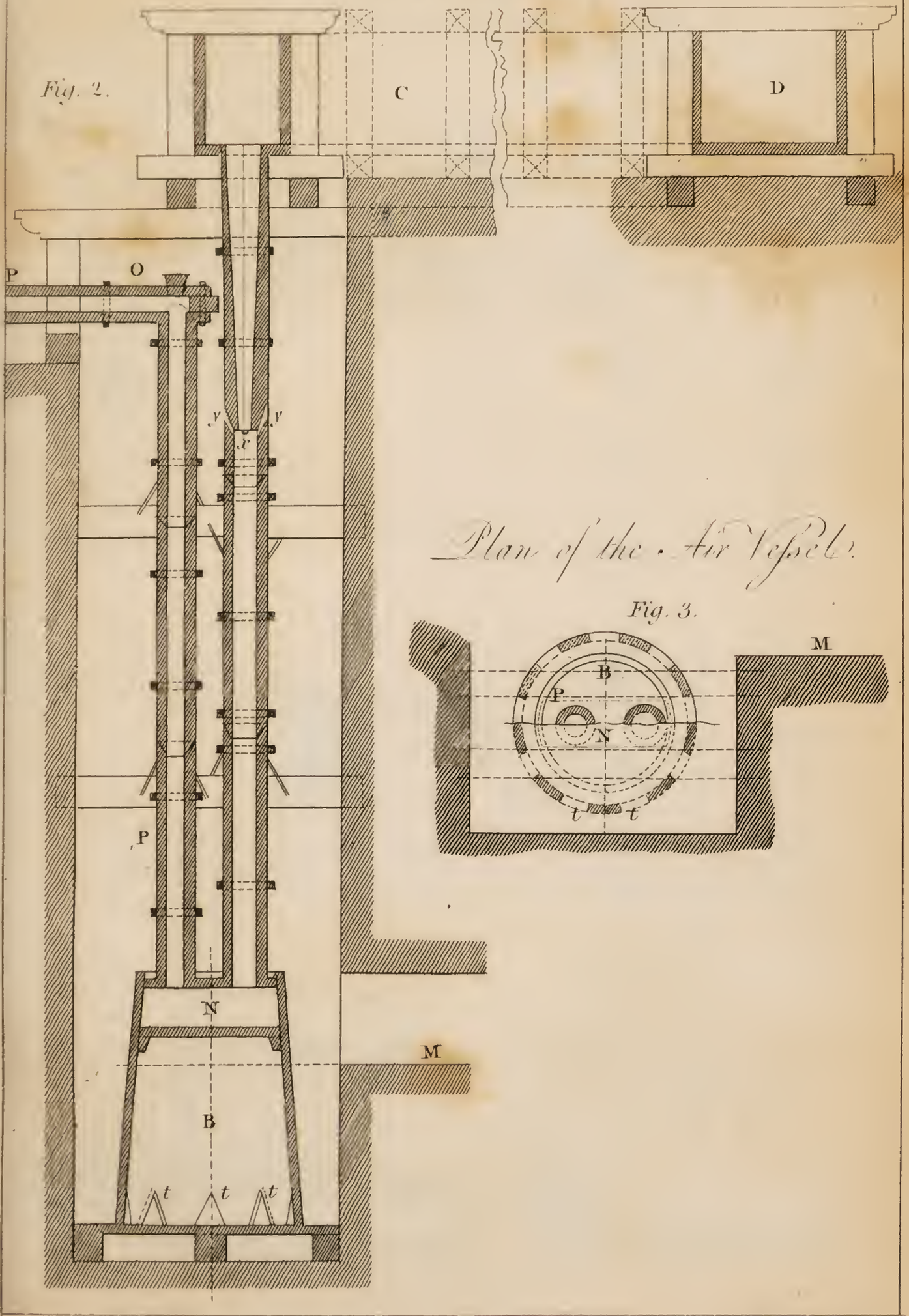


Mallow Sc. Rupsell Ct.

Blowing Engine by the fall of Water at Poullaouen.

Profil on the line 1.1.

Fig. 2.

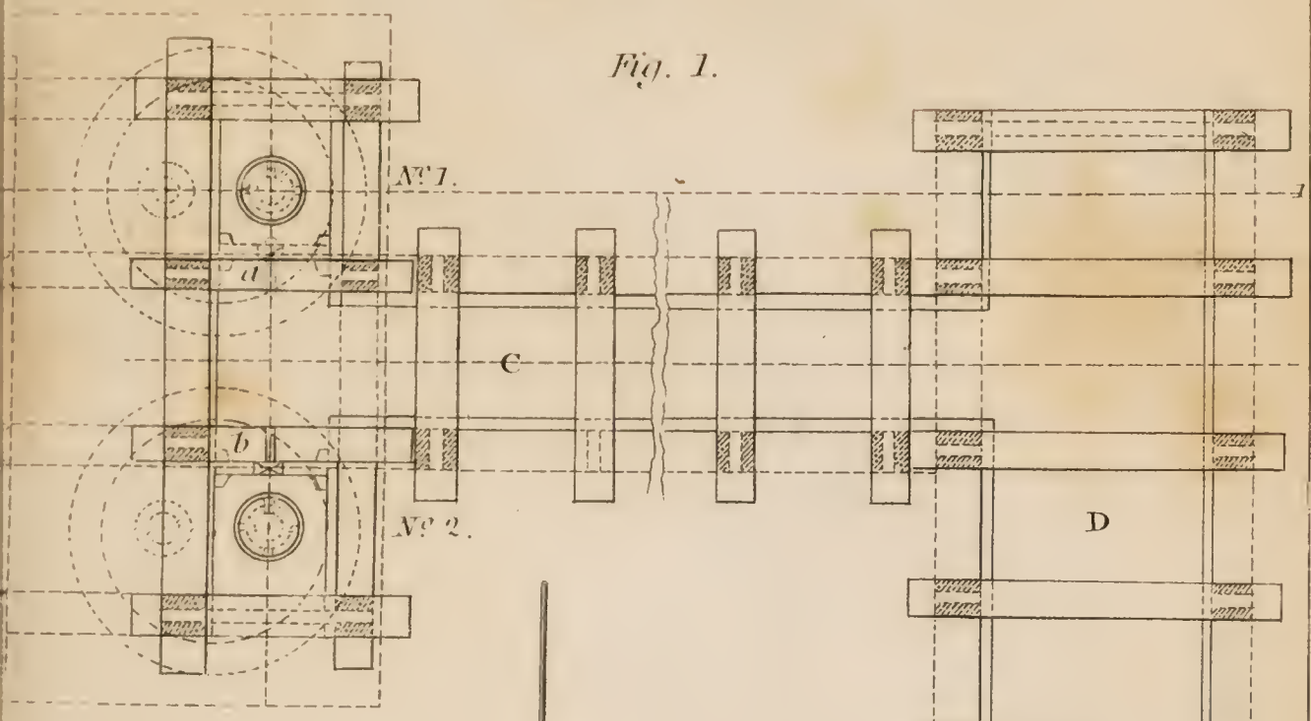


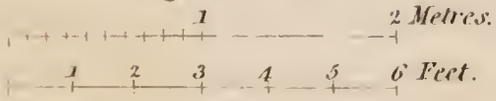
Plan of the Air Vessel.

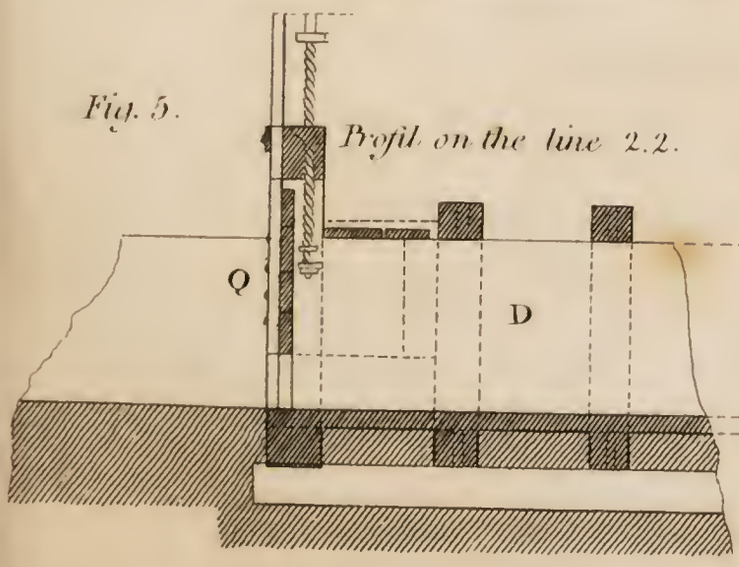
Fig. 3.

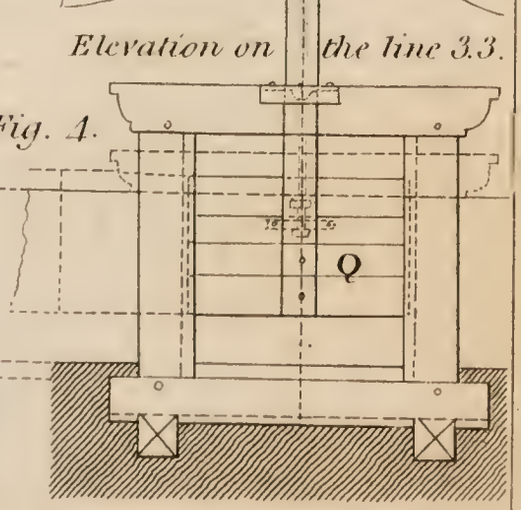
Mallow & Rupsell Sc.

Plan of the Water Ways.









A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

OCTOBER, 1805.

ARTICLE I.

Experimental Inveſtigations concerning Heat. By BENJAMIN COUNT OF RUMFORD, *V. P. R. S. Foreign Aſſociate of the National Inſtitute of France, &c. &c. Received from the Author.*

To Mr. NICHOLSON.

DEAR SIR, *Munich, Auguſt* 29, 1805.

HAVING learned by a letter which I received this day from England, that you have publiſhed in your Journal of Natural Philoſophy the memoir I ſent you on the temperature at which the denſity of water is a maximum, I take the liberty to ſend you herewith incloſed three memoirs on heat, which are deſtined to appear in the next volume of the publications of the firſt claſs of the National Inſtitute.—Three other memoirs of mine will appear in that volume, but as they contain little that would be new to you, I do not ſend them to you.

I continue my reſearches on heat, and have lately made ſeveral new and very intereſting experiments, the reſults of which it is my intention to communicate to you, as ſoon as I ſhall have compleated the particular courſe of experiments in which I am now engaged.

I am, Dear Sir, with much eſteem,
Your moſt obedient ſervant,
RUMFORD.

SECT. I. *Short Account of a new Experiment on Heat.*

I have lately made a new experiment, the result of which appears to me sufficiently interesting to deserve the attention of the class.

Qu. Whether the heating and cooling of polished and of blackened bodies follow the same law in small closed spaces as in spaces more enlarged?

Having found by experiments often repeated that metallic bodies exposed in the free air of a large apartment are much more speedily heated and cooled when their surfaces have been blackened (over the flame of a candle for example) than when they are clean and polished; I was curious to know whether the same phenomena would take place when, instead of exposing these bodies in the open air, they should be placed in close metallic vessels surrounded by a certain thickness of included air, and these vessels should be then plunged in a large mass of hot or cold water. In order to clear up this important point, I made the following experiment:

A cylindrical vessel of thin brass was supported in the middle of a larger vessel, so as to leave a thin interval of air between them.

A cylindrical vessel of brass, three inches in diameter and four inches long was enclosed in another larger cylindrical vessel, in the centre of which it was suspended by its neck, so as to touch it in no other part, leaving on all sides an interval of one inch between the vessels.

The external vessel as well as the smaller one included within it is made of thin sheets of brass; its diameter is five inches and its height six. It is one inch and a half in diameter and six inches high. Its neck is one inch and a quarter in diameter and two inches and a half long.

The interior vessel is suspended in the centre of the external one by a stopper of cork. This stopper is adjusted to the neck of the external vessel, and there is a cylindrical hole of three quarters of an inch diameter through the cork, and having the same axis, which perforation receives the neck of the interior vessel and retains it in its place.

The interior vessel was introduced and fixed in its place before the bottom of the exterior vessel was soldered in.

The larger vessel was supported on a foot.

At the centre of the bottom of the great vessel is a small metallic tube of three quarters of an inch diameter and one inch and a half long, by means of which this instrument is attached to a solid heavy foot of metal which supports it in a vertical position when the whole instrument is submerged in a vessel of water.

This instrument, which greatly resembles that described in my 7th essay on the propagation of heat in fluids, which I have called

called the Paſſage Thermometer*, may be uſed to make a number of intereſting experiments on the cooling of bodies through different fluids. In the preſent experiment I employed it in the following manner:

The interior veſſel was entirely filled with hot water to the height of half an inch in its neck, and a good thermometer, having its cylindric bulb four inches long, was inſerted therein. The inſtrument was then plunged in a mixture of pounded ice and water, and the time was noted by means of the thermometer, during which the hot water in the ſmall veſſel became cold.

The inner veſſel was filled with hot water, and a thermometer placed in its neck. The whole inſtrument was then plunged in ice and water.

I was careful to plunge the inſtrument in this frigorific mixture, ſo that the large veſſel was completely ſubmerged except the upper extremity of its neck; and I added from time to time a ſufficient quantity of pounded ice, to keep the frigorific mixture conſtantly and throughout at the temperature of melting ice.

The following were the reſults afforded by two ſimilar inſtruments employed at the ſame time:

Theſe two inſtruments, which I ſhall diſtinguiſh reſpectively by the letters A and B, are of the ſame form and dimenſions; there is no difference between them but in the ſtate of their ſurfaces. In the inſtrument A, the exterior ſurface of the ſmall veſſel and the interior ſurface of the great veſſel which incloſes it, are bright and poliſhed; but in the inſtrument B, the exterior ſurface of the ſmall veſſel and the interior ſurface of the large veſſel are black, having been blackened over the flame of a candle before the bottom of the great veſſel was ſoldered in its place.

Reſults with two inſtruments one of which, B, had the interior ſurface of the larger veſſel and the exterior of the ſmaller blacked; and the other inſtrument, A, had the like ſurfaces poliſhed.

Having filled the interior veſſel of each of theſe inſtruments with boiling water till the water roſe to the height of half an inch in the neck, I placed a thermometer in each, and then plunging both inſtruments at the ſame time into a tub filled with cold water mixed with pounded ice, I obſerved the courſe of their refrigeration during ſeveral hours.

The interior veſſels contained boiling water.

Each of the inſtruments was completely ſubmerged in the frigorific mixture, excepting about one inch of the ſuperior extremity of the neck of the exterior veſſel, and I was careful

The refrigerating veſſel contained ice and water.

* See our Journal, Vol. IX.

to add new quantities of pounded ice from time to time, in order to keep the frigorific mixture conſtantly at the preciſe temperature of melting ice.

Caution to inſure equality of temperature.

As the ſpecific gravity of water at the temperature of three or four degrees of the thermometer of Reaumur, is greater than that of melting ice, the water which lies at the bottom of a veſſel containing a mixture of water and pounded ice, is uſually warmer than the fluid which occupies the upper part of the veſſel. To remedy this inconvenience my refrigeratory for the frigorific mixture was a tin veſſel ſupported on three feet of one inch in length, and I placed this firſt veſſel in a larger of wood, containing a certain quantity of ice ſurrounding the bottom and part of the ſides of the metallic veſſel.

Method of obſervation.

As in the firſt moments of the experiment the thermometers deſcended too quickly to be obſerved with preciſion, I waited till each of them had arrived at the 55th degree of Reaumur; after which I carefully obſerved the number of minutes and ſeconds employed in paſſing through each interval of five degrees of the lower part of the ſcale of the thermometer to the fifth degree above zero.

Table of the courſe of cooling.

The following table exhibits the depreſſion of the thermometers during eight hours employed in the experiment.

Degrees of the Thermometer.	*Time employed in cooling,* By the Inſtrument A.	By the Inſtrument B.
From 55 to 50	11^{m} 6^{s}	7^{m} 50^{s}
50 45	13 15	8 10
45 40	15 12	9 5
40 35	19 10	10 50
35 30	22 24	12 18
30 25	27 50	15 10
25 20	37 6	21 15
20 15	54 15	28 15
15 10	80 25	41 25
10 5	183 45	85 15
Time employed in cooling from 55° to 5°,	478 4	254 5

It

It is evident from the refults of this experiment, that the blackened body is conftantly cooled in lefs time than the polifhed body; but it appears by the courfe of the thermometers, that the difference between the quickneſs of cooling of thefe two bodies varies, and that this difference was lefs conſiderable in proportion as the temperature of the bodies was more elevated in compariſon to that of the medium in which they were expofed to cool.

The blackened body always cooled more quickly than the other.

In cooling from the 55th degree to the 50th above the temperature of the ſurrounding medium, the poliſhed body employed $11^m\ 6^s$, and the blackened body employed $7^m\ 50^s$ to paſs through the ſame interval. But from the 10th to the 15th degree above the temperature of the medium, the poliſhed body employed $183^m\ 45^s$, while the blackened body employed only $85^m\ 15^s$; but it is extremely probable that this difference between the proportion of the times employed in cooling the two bodies at different temperatures, is only apparent, and that it depends on the greater or leſs time required for the thermometers in the veſſels to arrive at the mean temperatures of the maſſes of water which ſurround them.

The difference was greateſt at the loweſt temperatures; probably becauſe the thermometers then ſhewed the mean temperature more correctly.

In order to compare the refults of this experiment with thoſe I made laſt year with metallic veſſels poliſhed and blackened, and left to cool in the undiſturbed air of a large chamber, it is neceſſary to aſcertain how much time the two bodies in queſtion employed in cooling, from the 50th to the 40th degree of Fahrenheit above the temperature of the medium. Now I found by obſervation, that the poliſhed veſſel A employed $39^m\ 30^s$ to paſs over that interval of cooling, while the blackened veſſel B employed only 22^m. Theſe times are in the proportion of 10000 to 5810. By one of my experiments made laſt year, I found that the times employed in paſſing through the ſame interval of cooling in the open air by a clean poliſhed metallic veſſel, and another of the ſame form and capacity, but blackened without, were as 10000 to 5654.

From theſe experiments it appears that the rate of cooling in the poliſhed body, compared with the other, is nearly the ſame as was formerly determined with bodies in a large chamber.

Reflecting on the conſequences which ought to refult from the radiations of bodies, on the ſuppoſition that the temperatures of bodies are always changing by means of theſe radiations, I was led to the following concluſion: If the intenſity of the action of the rays which proceed from a body, be univerſally as the ſquares of the diſtances of bodies inverſely,

If the intenſities of radiated heat be inverſely as the ſquares of the diſtances, bodies will cool in the ſame time in an encloſure of the ſame tem-

perature, whether large or ſmall.

which is extremely probable, a hot body expoſed to cool in a cloſe place, or ſurrounded on all ſides by walls, ought to cool with the ſame celerity, or in the ſame time, whatever may be the magnitude of this encloſure, provided the temperature of the ſides or walls be at a conſtant given temperature; and the reſults of the experiment here deſcribed, in which the hot body was encloſed in a veſſel of a few inches diameter, compared with thoſe of ſeveral experiments made laſt year, in which the heated bodies expoſed to cool between the walls of a large chamber, appear to confirm this concluſion.

Theſe facts confirm that truth;

and that the air has little effect.

As to the effect produced by the air in cooling a heated body expoſed to cool in a cloſe place filled with that fluid, I have reaſon to believe that it is much leſs conſiderable than has been ſuppoſed.

Former experiments proved that it receives only 1-27th part.

I have ſhewn by direct and concluſive experiments, that bodies cool and are heated, and that with conſiderable celerity, when placed in a ſpace void of air *; and, by experiments made laſt year with the intention of clearing up this point, I found reaſons to conclude, that when a hot body cools in tranquil air not agitated by winds, one twenty-ſeventh only of the heat loſt by this body (or to ſpeak more correctly, which it excites in ſurrounding bodies) is communicated to the air, all the reſt being carried to a diſtance through the air, and communicated by radiation to the ſurrounding ſolid bodies.

The reſt paſſes by radiation.

Sect. II. *Experiments on cooling Bodies.*

It is only by careful obſervation of the phenomena which accompany the heating and cooling of bodies, that we can hope to acquire exact notions of the nature of heat and its manner of acting.

Conducting power of bodies with regard to heat.

Many experiments have been made by different perſons at different times, with a view to determine what has been called the conducting quality of different ſubſtances with regard to heat: I have myſelf made a conſiderable number; and it is from their reſults, often no leſs unexpected than intereſting, that I have been gradually led to adopt the opinions on the nature of heat which I have preſumed to ſubmit to the judgment of this illuſtrious aſſembly. The flattering attention

* See my Memoir on Heat in the Philoſophical Tranſactions for 1786, and in my eighth Eſſay.

with

with which the Claſs has honoured the three Memoirs I have lately preſented, encourages me to communicate the continuation of my reſearches.

All philoſophers are agreed in conſidering glaſs as one of the worſt conductors of heat which exiſts; and when it is propoſed to confine the heat in a body of which the temperature has been raiſed, or to hinder its diſſipation as much as poſſible, care is taken to ſurround the heated body with ſubſtances known to be bad conductors of heat.

Glaſs is allowed to be one of the worſt conductors.

The reſults of many of my experiments having led me to ſuſpect that the cooling of bodies is not effected in the manner which is generally ſuppoſed, I made the following experiment with the intention of clearing up this intereſting part of the ſcience.

Bodies are not, probably, cooled by conducting off.

I procured two bottles nearly cylindrical, of the ſame form and the ſame dimenſions when meaſured externally; one being of glaſs and very thick, and the other of tin or tinned iron, which was very thin. Each of them is three inches ten lines in diameter very nearly, and five inches in height, and each has a neck one inch three lines in diameter, and one inch two lines in height. The glaſs bottle weighs 13 ounces 1 gros and 18 grains poids de marc, and the other thin metallic veſſel weighs only 5 ounces 1 gros and 65 grains.

Experiment: A thick glaſs bottle and a thin one of tin,

Having very exactly weighed the bottle of tinned iron, I found its exterior ſurface to be 54,462 inches, which give 0,21142 of a line for the thickneſs of its ſides, taking the ſpecific gravity of the metal at 7,8404.

were prepared, weighed,

The mean thickneſs of the ſides of the glaſs bottle is more than ſix times as great, as may be eaſily deduced from a calculation founded on the weight of the bottle, the quantity of its ſurface, and the ſpecific gravity of glaſs.

and meaſured.

Having filled theſe two bottles with boiling water, I hung them up by ſlender ſtrings in the midſt of the tranquil air of a large chamber, at the height of five feet from the floor, and at the diſtance of four feet aſunder.

They were filled with hot water, and left to cool in the air.

The temperature of the air of the chamber, which did not vary a quarter of a degree during the whole time of the experiment, was $9\frac{3}{4}$ degrees of Reaumur's ſcale.

An excellent mercurial thermometer, with a cylindrical bulb, of four inches long and two lines and a half in diameter, ſuſpended in the axis of each of theſe bottles, indicated the temperature

temperature of the contained water; and the time employed in its cooling for every five degrees of Fahrenheit's thermometer, was carefully obſerved during eight hours.

The glaſs being conſidered as a very bad conductor of heat, and the ſides of the bottle being ſo thick, who would not have expected that the water in this bottle would have been more ſlowly cooled than that in the very thin bottle of tin.

The glaſs bottle cooled twice as quick as that of tin.

The contrary however was the event; the bottle of glaſs was cooled almoſt twice as quickly as that of tin.

While the water included in the bottle of tinned iron employed 56 minutes to paſs through a certain interval of cooling, namely through ten degrees, between the 50th and 40th degree of the thermometer of Fahrenheit above the temperature of the air of the chamber, the water in the glaſs bottle employed only 30 minntes for the ſame change.

Inference.

It appears to me, that the reſult of this experiment throws great light on the myſterious operation of the communication of heat.

Heat is not likely to be a material ſubſtance.

If we admit the hypotheſis that hot bodies are cooled, not by loſing or acquiring ſome material ſubſtance, but by the action of colder ſurrounding bodies, communicated by undulations or radiations excited in an etherial fluid, the reſults of this experiment may be eaſily explained; but if this hypotheſis be not adopted, I cannot explain them.

Bodies are not cooled by the ſurrounding air.

It might perhaps be ſuſpected that the air attached by a certain attraction, but with unequal forces, to the ſurfaces of the two bottles, might have been the cauſe of this remarkable difference in the time of their cooling; but thoſe who will take the trouble to reflect attentively on the reſults of the experiments I have deſcribed in a preceding memoir, which were made with a view to clear up this point, with a metallic veſſel firſt naked, and afterwards with one, two, four and five coatings of varniſh, will be perſuaded that this cauſe is not ſufficient to explain the facts.

All metallic veſſels have the ſame diſpoſition to cool.

By a courſe of experiments made at Munich laſt year, of which the details are given in a Memoir ſent to the Royal Society of London *, I have found that a given quantity of hot water included in a metallic veſſel of a given form and capacity, always cools with the ſame quickneſs in the air,

* See our Journal, Vol. IX. p. 194.

whatever

whatever may be the metal employed to conſtruct the veſſel; provided always that the external ſurface of the veſſel be very clean, and the temperature of the air the ſame.

The ſurface only need be metallic.

In order that the cooling ſhall be effected in the ſame time, nothing more is required than that the external ſurface of the veſſel be truly metallic, and not covered with oxide or other foreign bodies.

This ariſes from the opacity of metals,

On the enquiry, what quality all the metals might have in common, and poſſeſs in the ſame degree, to which this remarkable equality of their ſuſceptibility of cooling might be attributed, I found it in their opacity.

by which the heat is reflected,

The rays which cannot penetrate the ſurface of a body, muſt neceſſarily be thrown back or reflected; and as the rays of light, which have much analogy with the inviſible calorific or frigorific rays, eaſily penetrate glaſs, though they are reflected, at leaſt for the greateſt part, by metallic ſurfaces, I ſuſpected beforehand the reſult of the experiment with the two bottles, one of glaſs and the other of tinned iron.

Uſual compariſon of heat to water in a ſponge.

The ſtate of a heated body, or a body which contains a certain quantity of caloric, has been compared to that of a ſponge which contains a certain quantity of water. Supposing this compariſon to be juſt, we might compare the loſs of heat by the emiſſion of the calorific rays, to the loſs of water by evaporation. Let us try if this compariſon can ſupply us with the means of throwing ſome light on the intereſting ſubject of our reſearches.

The ſame amplified.

Inſtead of the ſponge filled with water, let us ſubſtitute the earth, and ſuppoſe for a moment that the earth is every where equally heated, and its ſurface in all parts covered with a bed of the ſame kind of ſoil equally moiſt.

If it were true, a rough ſurface would emit more heat than a ſmooth one.

As a ſquare league in a mountainous country contains more ſurface or more ſuperficial acres than a ſquare league ſituated in the plain, it is evident that more water would be evaporated from the whole ſurface of the earth in a given time if the earth were covered with mountains, than if its ſurface were an immenſe plain, and conſequently, that more caloric ought to be projected from the ſurface of any ſolid body broken with aſperités, than from the ſurface of another body of the ſame form and dimenſions, which is ſmooth or well poliſhed.

This

But the facts are contrary.

This reaſoning appears to me to be juſt, and if I am not deceived, the concluſions which may be drawn from the facts in queſtion, well confirmed by experiment, ought to be conſidered as demonſtrative. I have taken every poſſible care to eſtabliſh theſe facts; and the reſults of all my experiments have conſtantly ſhewn that more or leſs perfect poliſh, or the greater or leſs brightneſs of the ſurface of a metallic veſſel, does not ſenſibly influence the time of its cooling.

A poliſhed and unpoliſhed veſſel of braſs cooled in the ſame time.

I took two equal veſſels of braſs and poliſhed the external ſurface of one of them as highly as poſſible; and I deſtroyed the poliſh of the other by rubbing it in all directions with coarſe emery. When theſe two veſſels were filled with hot water, I did not find that the unpoliſhed veſſel employed more or leſs time in cooling than that which was poliſhed.

Caution.

I was careful to waſh the ſurface of the unpoliſhed veſſel effectually with water before the experiment; as I knew that if I did not take the precaution of removing all the dirt which might be lodged in the aſperités of the ſurface, the preſence of theſe ſmall foreign bodies would influence the reſult of the experiment in a ſenſible manner.

A rough ſurface may reflect as much light as a ſmoother.

We ought careſully to diſtinguiſh thoſe ſurfaces which appear unpoliſhed to our eyes, but which in fact are not ſo, from thoſe which reflect little or no light.

Metals not leſs reflective for loſing their poliſh.

It is more than probable that the ſurface of a metal is always poliſhed, and even always equally ſo in all the caſes wherein the metal is naked and clear and clean, notwithſtanding all the mechanical means which may be uſed to ſcratch its ſurface and break the glare of its luſtre.

If the radiation of heat deſcend on ſurrounding bodies, it will be of no conſequence whether the radiating body be poliſhed or not.

Let us return to the compariſon of the evaporation of water from the ſurface of the earth, with the emiſſion of caloric radiating from the ſurface of a heated body, and let us ſuppoſe for an inſtant, that the evaporation of the water from the ſurface of the earth does not depend on the heat of the earth itſelf, but that it is cauſed merely by the influences of ſurrounding bodies, as for example, by the rays of light received from the ſun. It is evident that, in this caſe, the evaporation could not be ſenſibly greater in a mountainous country than in the plain; and by an eaſy analogy we ſee, that if hot bodies be cooled, not in conſequence of the emiſſion of ſome material ſubſtance from their ſurfaces, but by the poſitive action of rays

ſent

ſent to them by colder ſurrounding bodies the more or leſs perfect poliſh of their ſurfaces, ought not ſenſibly to influence the rapidity of their cooling.

This is preciſely what all my experiments concur to prove. Experiments prove this poſition.

I have long ſought, and with that patience which the love of the ſciences inſpires, to reconcile the reſults of my experiments with the opinions generally received concerning the nature of heat and its mode of action, but without being able to ſucceed.

It is in the hands of two of the moſt illuſtrious bodies of learned men that ever exiſted that I have thought it incumbent on me to depoſit my labours, my diſcoveries, my doubts, and my conjectures.

I am earneſtly deſirous of engaging the philoſophers of all countries to turn their attention towards an object of enquiry too long neglected.

The ſcience of heat is not only of great curioſity, from the multitude of aſtoniſhing phenomena it offers to our contemplation, but it is likewiſe extremely intereſting from its intimate connection with all the uſeful arts, and generally with all the mechanical occupations of human life. Importance of the ſcience of heat.

Without a knowledge of heat it is not poſſible either to excite it with economy, or to direct its different operations with facility and preciſion.

(The Remainder in our next.)

II.

On pure Nickel, diſcovered to be one of the noble Metals, and on its Preparation and Properties. By J. B. Richter.*

On repeatedly cryſtallizing ſulphate of ammonia and nickel, the whole of the cobalt, an extremely ſmall quantity excepted, will be ſeparated; but after this there ſtill remains ſome copper mixed with the ſalt. I have already announced that this metal may be ſeparated from the nickel by ſubliming the latter with ſal ammoniac; but at that time I had never obtained pure nickel. With the compound ſalt of nickel and ammonia Cobalt ſeparated from ſulphate of ammonia and nickel by repeated cryſtallization. Copper, by ſubliming with ſal ammoniac.

* Tranſlated from the *Journal de Chimie* of Van Mons, vol. VI. p. 183, March, 1805; and abridged by Van Mons from the *Allgemeines Journal der Chemie*, 1804, vol. III. p. 244.

Arſenic and iron left.

a little arſenic ſtill remains; and there may be iron likewiſe, when we have been a little too ſparing in the addition of nitric acid to the ſulphuric ſolution of cobalt containing calx of nickel.

The triple ſalt decompoſed by carbonate of potaſh.

I endeavoured to ſeparate theſe extraneous metals in the humid way, but not with complete ſucceſs. I decompoſed by means of carbonate of potaſh the triple ammoniacal ſalt of nickel, free from iron, and as much as poſſible from cobalt: the precipitate ſtill was viſibly of a greeniſh blue. Having edulcorated it, and heated it to redneſs, it changed its colour, as it loſt its carbonic acid, to a blackiſh gray, which however inclined evidently to a green. The water of edulcoration, which had a greeniſh appearance, was evaporated to dryneſs, and the reſiduum, after being heated red hot, was waſhed again. A green powder remained, which did not loſe its colour in the fire, and conſiſted in great part of arſeniate of nickel.

The metal reduced.

I mixed each of the two reſiduums ſeparately with a fifth part of charcoal, and expoſed them to the heat of a potter's furnace for eighteen hours in a cupel with a little porcelain glaze. The metallic buttons obtained differed a little from each other. Each endured a few blows with a hammer without cracking; but that of the latter reſiduum was much more white and fragile than that of the former, the colour of which approached that of ſteel and was ſlightly reddiſh. They were both attacked with avidity by nitric acid, and they were attracted by the magnet, but the former only weakly.

The reſult not pure nickel.

As from ſeveral effects on porcelain it appeared to me probable, that pure nickel was a noble metal, I diſſolved afreſh in nitric acid the whole quantity reduced, which amounted to ſeveral ounces, and evaporated the ſolution to dryneſs. I then poured water on the ſaline maſs, and a beautiful green ſolution was formed; but a greeniſh white reſiduum remained, in which I eaſily detected the preſence of iron, nickel, and arſenic acid.

The ſolution precipitated and expoſed to a ſtrong heat.

The ſolution, which beſide arſenic, contained a conſiderable portion of copper, was precipitated by carbonate of potaſh, and the reſiduum, the colour of which was ſtill very lively, though not ſo green as that of carbonate of copper, was well waſhed and expoſed to a white heat. This changed its apple green colour to a deep green inclining to gray and brown. With a ſtronger heat the maſs aſſumed a grayer brown, and at

the

the ſame time appeared to coagulate. There were likewiſe portions of reduced metal in it, that could not be miſtaken.—I could not, however, accompliſh its fuſion in a wind-furnace, ſurmounted with a cupellating furnace dome, and having a long chimney. In conſequence, I divided it into ſeveral portions, which I expoſed in crucibles to the ſtrongeſt heat of a potter's furnace, in which capſules of the moſt refractory clay are frequently ſoftened. Very refractory.

In thoſe crucibles which were placed where the porcelain is longeſt taking, the matter had experienced no change but a coagulation. In the other crucibles it had entered into complete fuſion, yet not into a liquid fuſion, and the crucibles had partly experienced the ſame effect. Here and there in the melted maſs metallic globules were found, the largeſt of which were the ſize of a ſmall nut, and the leaſt that of a cherry-ſtone. Their brilliancy was a mean between that of ſilver and that of Engliſh tin. The ſcoriæ were greeniſh brown, mixed with an amethyſt colour, and in ſome places a deep blue entirely like fuſed oxide of cobalt. The brown colour aroſe from the oxide of copper, which was completely vitrified, and the blue from that of cobalt. The green, on the contrary, proceeded from arſeniate of nickel, which, as I have learned by experience, ſtrongly reſiſts fuſion, without the addition of ſome combuſtible ſubſtance. Fuſed in the ſtrongeſt heat of a potter's furnace. Scoriæ. Arſeniate of nickel.

I attempted to hammer the metallic globules on an anvil, and to my great ſatisfaction I found that they poſſeſſed conſiderable malleability. They were eagerly attracted by the magnet. The metal malleable, and magnetic.

As it was impoſſible to ſeparate the ſcorie with the hammer from the little globules to which they adhered, I collected them together by trituration and decantation, and expoſed them to fuſion afreſh. It was again complete only in the places of the furnace moſt heated. Re-fuſion.

Convinced from theſe reſults, that nickel is reducible in the fire without the addition of any combuſtible matter, I attempted to reduce ſome oxide of this metal, obtained by the decompoſition of the triple ammoniacal ſalt of nickel, which during an uninterrupted labour of eighteen months, I had procured in a very large quantity. On this occaſion the ſame phenomena occurred as in the preceding reductions. Reduced without any addition.

I repeated the melting till the metal had undergone a complete fuſion, and was found collected together in a button at the A button of an ounce and half procured.

the bottom of the crucible. In one crucible which had been expoſed to the ſtrongeſt heat, I obtained a button that weighed an ounce and a half.

Beſt reduced alone.

I was leſs ſucceſsful in my fuſion when I mixed the oxide of nickel with porcelain glaze, or when I ſimply covered it with this glaze; ſo that I was convinced the beſt proceſs was to reduce the oxide of nickel directly.

After much time and patience, I ſucceeded in obtaining ſeveral ounces of nickel, which I muſt conſider as abſolutely pure, and I ſhall now proceed to deſcribe the principal characters that I have perceived in it in this ſtate.—To begin with the external characters.

External character of pure nickel.

The colour of pure nickel is a mean between ſilver and tin.

Colour.

Unchangeable in the air.

It undergoes no alteration either from the air of the atmoſphere or from the water in it: in other words it is inſuſceptible of being oxided by the air.

Malleable.

It is perfectly malleable; as it may not only be forged into bars when red hot, but hammered on the anvil while cold into very thin plates. This character removes nickel from the claſs of ſemi-metals to that of perfect metals.

Specific gravity.

Its denſity or ſpecific gravity is pretty conſiderable: from repeated experiments with my hidrometer caſt nickel weighs 8.279, and forged nickel 8.666.

Ductile.

Its tenacity likewiſe appears conſiderable, to judge from its great ductility. A piece of caſt nickel, weighing five drams allowed itſelf to be flattened, but not without cracking, into a plate of 13 ſquare inches Rhyniand meaſure, which gives leſs than $\frac{1}{100}$ of an inch for its thickneſs. It might probably be drawn into a wire of the ſame tenuity.

Refractory.

The reſiſtance of nickel to fuſion is very conſiderable, and equals, if it do not ſurpaſs, that of manganeſe. The reader may have obſerved, from my attempts to fuſe it, how difficult it is to obtain any thing deciſive on this head.

A noble metal.

At a temperature ſufficiently high the pure oxide of nickel is reducible without the addition of any combuſtible matter. Its great reſiſtance to fuſion is the only cauſe why this reduction preſents ſo many difficulties. Very little oxidation too is perceptible on keeping this metal in a ſtate of incandeſcence: it is merely tarniſhed a little more than platina, gold, or ſilver. Nickel therefore belongs not to the claſs of perfect metals merely, but to that of noble metals.

Magnetic.

The action of the magnet on nickel is not only very conſi-

derable

derable, and little inferior to that on iron; but nickel becomes itſelf magnetical, or acquires polarity, by the touch, and even in part by ſtriking it with a hammer, or filing it, with the precautions ſuitable for producing this effect. I diſcovered the latter property by preſenting to the magnet a ſlip of forged nickel; when, notwithſtanding it was poliſhed by the file, it adhered more feebly to the magnet than other ſlips leſs poliſhed; but on my preſenting its other extremity to the magnet, it adhered to it with great force. It likewiſe attracted by either ſide not only iron needles, but plates of nickel half an inch ſquare, which it cauſed to move about on a ſmooth table.

Its magnetic property weakened by copper, deſtroyed by arſenic. Magnetic and malleable in proportion to its purity.

The property which nickel poſſeſſes of becoming magnetic is not deſtroyed, though weakened by its alloy with copper; but arſenic deſtroys it completely. I had frequent opportunities of making this obſervation in the courſe of my experiments. Some nickel, from which I had ſeparated the iron * and the arſenic in the humid way, and which I had afterwards reduced with the addition of a combuſtible ſubſtance, was malleable, and attracted the magnet, though not ſo forcibly as pure nickel. The ſame metal, purified with leſs care, was leſs malleable, and proportionally leſs attractable by the magnet. Repeated expoſure of the metal to the moſt powerful heat of a porcelain furnace did not in the leaſt reſtore to it this property.—Some experiments, which I ſhall hereafter relate, have convinced me, that copper cannot be entirely ſeparated from nickel in the humid way, and that the only means of ſeparating them is to reduce the cupreous oxide of nickel by fire.

Copper muſt be ſeparated by fire.

Action of the acids upon nickel.

The ſulphuric and muriatic acids have little action upon nickel. The oxide of this metal by the air does not diſſolve even in the latter of theſe acids without the aſſiſtance of a ſtrong ebullition. The moſt appropriate ſolvents of nickel are the nitric and nitro-muriatic acids. I have already mentioned, that impure nickel, particularly the cupreous is attacked by the nitric acid with heat and vivacity. The action of the ſame acid on pure nickel is a little different, and particularly on the

Beſt mode of freeing it from iron.

* The ſeparation of the iron ſucceeds beſt by a rapid evaporation of the nitric ſolution of the ferruginous nickel, by which the iron is precipitated in the form of an inſoluble oxide. At the ſame time a little arſenic is ſeparated in union with the iron. It is preferable, however, to ſeparate the arſenic firſt, which is effected by the help of a nitric ſolution of lead. The lead is afterward to be precipitated by a ſolution of ſulphate of potaſh.

hammered

hammered metal. I have poured nitric acid on nickel both in buttons and laminated, expecting a very active ſolution; but it has proceeded ſlowly, and I have even been obliged to have recourſe to the heat of a ſpirit lamp to accelerate it. The diſſolution however having appeared to ceaſe, I decanted the liquid and poured on the reſiduum a freſh quantity of acid of the ſame ſtrength as the preceding, when on a ſudden ſuch a briſk action came on, accompanied with the evolution of heat *, that I could not remove the capſule to the fire-place quickly enough.

I ſhall now go on to conſider ſome of the characters of pure nickel in the ſtate of oxidation.

Characters of oxide of nickel.

The nitric ſolution of pure nickel has a beautiful graſs-green colour. Carbonate of potaſh ſeparates from it a pale apple green precipitate. This precipitate well waſhed and dried is very light. A thouſand parts of metallic nickel reduced to this precipitate weigh 2,927 parts.

Precipitate very light.

If this precipitate be expoſed to a white heat it becomes of a blackiſh gray, ſcarcely inclining to green, and weighing only 1,285. On continuing the fire, the maſs approaches the metallic ſtate more and more, and becomes magnetic. This is effected much more ſpeedily if the oxide be moiſtened with a little oil.

Oil promotes its reduction.

Precipitate by ammonia.

On adding cauſtic ammonia in exceſs to a nitric ſolution of nickel, a precipitate is formed, reſembling in colour ammoniure of copper, but not ſo deep. This colour ſometimes changes in a couple of hours to an amethyſt red, and to a violet, which colours are converted into an apple-green on the addition of an acid, and again to a blue and violet on the addition of ammonia. If however we add to the ſolution of nickel a ſolution of copper, ſo as to produce no perceptible change, the colour of the precipitate formed by ammonia ceaſes to aſſume a red tinge, and the red colour of the ammoniure of nickel diſappears on the addition of a little ammoniure of copper; whence it follows, that every precipitate of nickel by ammonia, which retains its blue colour, has copper combined with it.

Its being a noble metal queſtioned.

* From this it is difficult to believe that nickel, under favourable circumſtances, would not become oxided by the combined influence of air and fire. VAN MONS.

On

III.

On the Analyſis of Soils, as connected with their Improvement.
By HUMPHREY DAVY, *Eſq. F. R. S. Profeſſor of Chemiſtry to the Board of Agriculture and to the Royal Inſtitution.*

I. *Utility of Inveſtigation relating to the Analyſis of Soils.*

THE methods of improving lands are immediately connected with the knowledge of the chemical nature of ſoils, and experiments on their compoſition appear capable of many uſeful applications.

Analyſis of ſoils; attended to by Mr. Young, Lord Dundonald, and Mr. Kirwan.

The importance of this ſubject has been already felt by ſome very able cultivators of ſcience; many uſeful facts and obſervations with regard to it have been furniſhed by Mr. Young; it has been examined by Lord Dundonald, in his treatiſe on the connexion of Chemiſtry with Agriculture, and by Mr. Kirwan in his excellent eſſay on Manures: but the enquiry is ſtill far from being exhauſted, and new methods of elucidating it are almoſt continually offered, in conſequence of the rapid progreſs of chemical diſcovery.

In the following pages I ſhall have the honour of laying before the Board, an account of thoſe methods of analyſing ſoils which appear moſt preciſe and ſimple, and moſt likely to to be uſeful to the practical farmer; they are founded partly upon the labours of the gentlemen, whoſe names have been juſt mentioned, and partly upon ſome later improvements.

II. *Of the Subſtances found in Soils.*

Soils contain earths, animal and vegetable remains, ſaline compounds, and oxide of iron.

The ſubſtances which are found in ſoils, are certain mixtures or combinations of ſome of the primitive earths, animal and vegetable matter in a decompoſing ſtate, certain ſaline compounds, and the oxide of iron. Theſe bodies always retain water, and exiſt in very different proportions in different lands; and the end of analytical experiments is the detection of their quantities and mode of union.

The *earths* found in common ſoils are principally ſilex or the earth of flints, alumine or the pure matter of clay, lime, or calcareous earth, and magneſia.

Silex.

Silex, or the earth of flints, when perfectly pure, appears in the form of a white powder, which is incombuſtible, infuſible,

inſoluble in water, and not acted upon by common acids; it is the ſubſtance which conſtitutes the principal part of rock chryſtal; it compoſes a conſiderable part of hard gravelly ſoils, of hard ſandy ſoils, and of hard ſtony lands.

Alumine. *Alumine*, or pure clay, in its perfect ſtate is white like ſilex; it adheres ſtrongly to the tongue, is incombuſtible, inſoluble in water, but ſoluble in acids, and in fixed alkaline menſtrua. It abounds moſt in clayey ſoils and clayey loams; but even in the ſmalleſt particles of theſe ſoils it is uſually united to ſilex and oxide of iron.

Lime. *Lime* is the ſubſtance well known in its pure ſtate under the name of quicklime. It always exiſts in ſoils in combination, and that principally with fixed air or carbonic acid, when it is called carbonate of lime; a ſubſtance which in the moſt compact form conſtitutes marble, and in its looſer form chalk. Lime, when combined with ſulphuric acid (oil of vitriol), produces ſulphate of lime (gypſum), and with phoſphoric acid, phoſphate of lime. The carbonate of lime, mixed with other ſubſtances, compoſes chalky ſoils and marles, and it is found in ſoft ſandy ſoils.

Magneſia. *Magneſia*, when pure, appears as white, and in a lighter powder, than any of the other earths; it is ſoluble in acid, but not in alkaline menſtrua; it is rarely found in ſoils; when it does exiſt, it is either in combination with carbonic acid, or with ſilex and alumine.

Animal decompoſing matter. *Animal decompoſing matter* exiſts in very different ſtates, according as the ſubſtances from which it is produced are different; it contains much carbonaceous ſubſtance, and may be principally reſolved by heat into this ſubſtance, volatile alkali, inflammable aeriform products, and carbonic acid; it is principally found in lands that have been lately manured.

Vegetable decompoſing matter. *Vegetable decompoſing matter* is likewiſe very various in kind, it contains uſually more carbonaceous ſubſtance than animal matter, and differs from it in the reſults of its decompoſition principally in not producing volatile alkali; it forms a great proportion of all peats; it abounds in rich mould, and is found in larger or ſmaller quantities in all lands.

Saline compounds. The *ſaline compounds* found in ſoils are very few, and in quantities ſo ſmall, that they are rarely to be diſcovered. They are principally muriate of ſoda (common ſalt), ſulphate of magneſia (Epſom ſalt), and muriate and ſulphate of potaſh, nitrate of lime, and the mild alkalies.

The

The *oxide of iron* is the ſame with the ruſt produced by expoſing iron to the air and water; it is found in all ſoils, but is moſt abundant in yellow and red clays, and in yellow and red ſiliceous ſands. Oxide of iron.

A more minute account of theſe different ſubſtances would be incompatible with the object of this paper. A full deſcription of their properties and agencies may be found in the elementary books on chemiſtry, and particularly in the Syſtem of Chemiſtry by Dr. Thomſon (2d Ed.); and in Henry's Epitome of Chemiſtry.

III. *Inſtruments required for the Analyſis of Soils.*

The really important inſtruments required for the analyſis of ſoils are few, and but little expenſive. They are a balance capable of containing a quarter of a pound of common ſoil, and capable of turning when loaded, with a grain; a ſeries of weights from a quarter of a pound Troy to a grain; a wire ſieve, ſufficiently coarſe to admit a pepper corn through its apertures; an Argand lamp and ſtand; ſome glaſs bottles; Heſſian crucibles; porcelain, or queen's ware evaporating baſons; a Wedgewood peſtle and mortar; ſome filters made of half a ſheet of blotting paper, folded ſo as to contain a pint of liquid, and greaſed at the edges; a bone knife, and an apparatus for collecting and meaſuring aeriform fluids. Inſtruments for analyſis. A balance, weights, ſieve, lamp, bottles, crucibles, baſons, p. and mortar, filters, knife, app. for gaſes.

The chemical ſubſtances or reagents required for ſeparating the conſtituent parts of the ſoil, are muriatic acid (ſpirit of ſalt), ſulphuric acid, pure volatile alkali diſſolved in water, ſolution of pruſſiate of potaſh, ſoap lye, ſolution of carbonate of ammoniac, of muriate of ammonia, ſolution of neutral carbonate of potaſh, and nitrate of ammoniac. An account of the nature of theſe bodies, and their effects, may be found in the chemical works already noticed; and the reagents are ſold together with the inſtruments mentioned above, by Mr. Knight, Foſter Lane, Cheapſide, arranged in an appropriate cheſt. Re-agents. Mur. and ſulp. acids, vol. alk. pr. potaſh, ſoap lye, carb. amm. mur. amm. carb. pot. nitr. amm.

IV. *Mode of collecting Soils for Analyſis.*

In caſes when the general nature of the ſoil of a field is to be aſcertained, ſpecimens of it ſhould be taken from different places, two or three inches below the ſurface, and examined as to the ſimilarity of their properties. It ſometimes happens, that upon plains the whole of the upper ſtratum of the land is How ſamples of ſoils are to be collected.

of the ſame kind, and in this caſe, one analyſis will be ſufficient; but in vallies, and near the beds of rivers, there are very great differences, and it now and then occurs that one part of a field is calcareous, and another part ſiliceous; and in this caſe, and in analogous caſes, the portions different from each other ſhould be ſeparately ſubmitted to experiment.

and preſerved if needful.

Soils when collected, if they cannot be immediately examined, ſhould be preſerved in phials quite filled with them, and cloſed with ground glaſs ſtoppers.

The quantity of ſoil moſt convenient for a perfect analyſis, is from two or four hundred grains. It ſhould be collected in dry weather, and expoſed to the atmoſphere till it becomes dry to the touch.

The ſpecific gravity

The ſpecific gravity of a ſoil, or the relation of its weight to that of water, may be aſcertained by introducing into a phial, which will contain a known quantity of water, equal volumes of water and of ſoil, and this may be eaſily done by pouring in water till it is half full, and then adding the ſoil till the fluid riſes to the mouth; the difference between the weight of the ſoil and that of the water, will give the reſult. Thus if the bottle contains four hundred grains of water, and gains two hundred grains when half filled with water and half with ſoil, the ſpecific gravity of the ſoil will be 2, that is, it will be twice as heavy as water, and if it gained one hundred and ſixty-five grains, its ſpecific gravity would be 1825, water being 1000.

is of importance to be known.

It is of importance, that the ſpecific gravity of a ſoil ſhould be known, as it affords an indication of the quantity of animal and vegetable matter it contains; theſe ſubſtances being always moſt abundant in the lighter ſoils.

Other phyſical properties.

The other phyſical properties of ſoils ſhould likewiſe be examined before the analyſis is made, as they denote, to a certain extent, their compoſition, and ſerve as guides in directing the experiments. Thus ſiliceous ſoils are generally rough to the touch, and ſcratch glaſs when rubbed upon it; aluminous ſoils adhere ſtrongly to the tongue, and emit a ſtrong earthy ſmell when breathed on; and calcareous ſoils are ſoft, and much leſs adheſive than aluminous ſoils.

V. *Mode of aſcertaining the Quantity of Water of Abſorption in Soils.*

Soils, though as dry as they can be made by continued expoſure to air, in all caſes ſtill contain a conſiderable quantity of water, which adheres with great obſtinacy to the earths and animal and vegetable matter, and can only be driven off from them by a conſiderable degree of heat. The firſt proceſs of analyſis is, to free the given weight of ſoil from as much of this water as poſſible, without in other reſpects, affecting its compoſition; and this may be done by heating it for ten or twelve minutes over an Argand's lamp, in a baſon of porcelain, to a temperature equal to 300 * Fahrenheit; and in caſe a thermometer is not uſed, the proper degree may be eaſily aſcertained, by keeping a piece of wood in contact with the bottom of the diſh; as long as the colour of the wood remains unaltered, the heat is not too high; but when the wood begins to be charred, the proceſs muſt be ſtopped. A ſmall quantity of water will perhaps remain in the ſoil even after this operation, but it always affords uſeful comparative reſults; and if a higher temperature were employed, the vegetable or animal matter would undergo decompoſition, and in conſequence the experiment be wholly unſatisfactory.

Evaporation of the abſorbed water, to ſhew its quantity in ſoils.

The loſs of weight in the proceſs ſhould be carefully noted, and when in four hundred grains of ſoil it reaches as high as 50, the ſoil may be conſidered as in the greateſt degree abſorbent, and retentive of water, and will generally be found to contain a large proportion of aluminous earth. When the loſs is only from 20 to 10, the land may be conſidered as only ſlightly abſorbent and retentive, and the ſilicious earth as moſt abundant.

One-eighth is an extreme proportion.

VI. *Of the Separation of Stones, Gravel, and vegetable Fibres from Soils.*

None of the looſe ſtones, gravel, or large vegetable fibres ſhould be divided from the pure ſoil till after the water is drawn off; for theſe bodies are themſelves often highly abſorbent and retentive, and in conſequence influence the fertility of the land.

Stones, &c. to be ſeparated after the drying, &c.

* In ſeveral experiments, in which this proceſs has been carried on by diſtillation, I have found the water that came over pure, and no ſenſible quantity of other volatile matter was produced.

The

The next proceſs, however, after that of heating, ſhould be their ſeparation, which may be eaſily accompliſhed by the ſieve, after the ſoil has been gently bruiſed in a mortar. The weights of the vegetable fibres or wood, and of the gravel and ſtones ſhould be ſeparately noted down, and the nature of the laſt aſcertained; if calcareous, they will effervefce with acids; if ſiliceous, they will be ſufficiently hard to ſcratch glaſs; and if of the common aluminous claſs of ſtones, they will be ſoft, eaſily ſcratched with a knife, and incapable of effervefcing with acids.

VII. *Separation of the Sand and Clay, or Loam, from each other.*

Sand, clay, and loam ſeparated from each other by elutriation.

The greater number of ſoils, beſides gravel and ſtones, contain larger or ſmaller proportions of ſand of different degrees of fineneſs; and it is a neceſſary operation, the next in the proceſs of analyſis, to detach them from the parts in a ſtate of more minute diviſion, ſuch as clay, loam, marle, and vegetable and animal matter. This may be effected in a way ſufficiently accurate, by agitation of the ſoil in water. In this caſe, the coarſe ſand will generally ſeparate in a minute, and the finer in two or three minutes, whilſt the minutely divided earthy, animal, or vegetable matter will remain in a ſtate of mechanical ſuſpenſion for a much longer time; ſo that by pouring the water from the bottom of the veſſel, after one, two, or three minutes, the ſand will be principally ſeparated from the other ſubſtances, which, with the water containing them, muſt be poured into a filter, and after the water has paſſed through, collected, dried and weighed. The ſand muſt likewiſe be weighed, and their reſpective quantities noted down. The water of lixiviation muſt be preſerved, as it will be found to contain the ſaline matter, and the ſoluble animal or vegetable matters, if any exiſt in the ſoil.

VIII. *Examination of the Sand.*

The ſand ſeparated into ſiliceous and calcareous.

By the proceſs of waſhing and filtration, the ſoil is ſeparated into two portions, the moſt important of which is generally the finely divided matter. A minute analyſis of the ſand is ſeldom or never neceſſary, and its nature may be detected in the ſame manner as that of the ſtones or gravel. It is always either ſilicious ſand, or calcareous ſand, or a mixture of both. If it

conſiſts

conſiſts wholly of carbonate of lime, it will be rapidly ſoluble in muriatic acid, with effervescence; but if it conſiſt partly of this ſubſtance, and partly of ſiliceous matter, the reſpective quantities may be aſcertained by weighing the reſiduum after the action of the acid, which muſt be applied till the mixture has acquired a ſour taſte, and has ceaſed to effervesce. This reſiduum is the ſilicious part: it muſt be waſhed, dried, and heated ſtrongly in a crucible; the difference between the weight of it and the weight of the whole, indicates the proportion of calcareous ſand.

IX. *Examination of the finely divided Matter of Soils, and Mode of detecting mild Lime and Magneſia.*

The finely divided matter of the ſoil is uſually very compound in its nature; it ſometimes contains all the four primitive earths of ſoils, as well as animal and vegetable matter; and to aſcertain the proportions of theſe with tolerable accuracy, is the moſt difficult part of the ſubject. The finely divided matter treated.

The firſt proceſs to be performed, in this part of the analyſis, is the expoſure of the fine matter of the ſoil to the action of the muriatic acid. This ſubſtance ſhould be poured upon the earthy matter in an evaporating baſon, in a quantity equal to twice the weight of the earthy matter; but diluted with double its volume of water. The mixture ſhould be often ſtirred, and ſuffered to remain for an hour or an hour and a half before it is examined. Muriatic acid takes up lime, magneſia, or iron.

If any carbonate of lime or of magneſia exiſt in the ſoil, they will have been diſſolved in this time by the acid, which ſometimes takes up likewiſe a little oxide of iron; but very ſeldom any alumine.

The fluid ſhould be paſſed through a filter; the ſolid matter collected, waſhed with rain water, dried at a moderate heat, and weighed. Its loſs will denote the quantity of ſolid matter taken up. The waſhings muſt be added to the ſolution, which if not ſour to the taſte, muſt be made ſo by the addition of freſh acid, when a little ſolution of common pruſſiate of potaſh muſt be mixed with the whole. If a blue precipitate occurs, it denotes the preſence of oxide of iron, and the ſolution of the pruſſiate muſt be dropped in till no farther effect is produced. To aſcertain its quantity, it muſt be collected in the ſame manner as other ſolid precipitates, and heated red; the reſult is oxide of iron. Precip. of iron (if preſent) by pruſſ. alkali;

Into

and earth, by carbonate of potaſh.

Into the fluid freed from oxide of iron, a ſolution of neutralized carbonate of potaſh muſt be poured till all effervescence ceaſes in it, and till its taſte and ſmell indicate a conſiderable exceſs of alkaline ſalt.

The precipitate that falls down is carbonate of lime; it muſt be collected on the filter, and dried at a heat below that of redneſs.

The remaining fluid muſt be boiled for a quarter of an hour, when the magneſia, if any exiſt, will be precipitated from it, combined with carbonic acid, and its quantity is to be aſcertained in the ſame manner as that of the carbonate of lime.

Alumine if taken up.

If any minute proportion of alumine ſhould, from peculiar circumſtances, be diſſolved by the acid, it will be found in the precipitate with the carbonate of lime, and it may be ſeparated from it by boiling for a few minutes with ſoap lye, ſufficient to cover the ſolid matter. This ſubſtance diſſolves alumine, without acting upon carbonate of lime.

Carbonate of lime if in plenty, may be eſtimated by the quantity of carbonic acid.

Should the finely divided ſoil be ſufficiently calcareous to effervesce very ſtrongly with acids, a very ſimple method may be adopted for aſcertaining the quantity of carbonate of lime, and one ſufficiently accurate in all common caſes.

Carbonate of lime, in all its ſtates, contains a determinate proportion of carbonic acid, *i. e.* about 45 per cent. ſo that when the quantity of this elaſtic fluid, given out by any ſoil during the ſolution of its calcareous matter in an acid is known, either in weight or meaſure, the quantity of carbonate of lime may be eaſily diſcovered.

When the proceſs by diminution of weight is employed, two parts of the acid and one part of the matter of the ſoil muſt be weighed in two ſeparate bottles, and very ſlowly mixed together till the effervescence ceaſes; the difference between their weight before and after the experiment, denotes the quantity of carbonic acid loſt; for every four grains and a half of which, ten grains of carbonate of lime muſt be eſtimated.

The beſt method of collecting the carbonic acid, ſo as to diſcover its volume, is by the pneumatic apparatus, the conſtruction and application of which is deſcribed at the end of this paper. The eſtimation is, for every ounce meaſure of carbonic acid, two grains of carbonate of lime.

X. *Mode*

X. *Mode of aſcertaining the Quantity of inſoluble finely divided Animal and Vegetable Matter.*

After the fine matter of the ſoil has been acted upon by muriatic acid, the next proceſs is to aſcertain the quantity of finely divided inſoluble animal and vegetable matter that it contains.

Ignition in an open veſſel deſtroys vegetable and animal matters.

This may be done with ſufficient preciſion, by heating it to ſtrong ignition in a crucible over a common fire till no blackneſs remains in the maſs. It ſhould be often ſtirred with a metallic wire, ſo as to expoſe new ſurfaces continually to the air; the loſs of weight that it undergoes denotes the quantity of the ſubſtance that it contains deſtructible by fire and air.

It is not poſſible to aſcertain whether this ſubſtance is wholly animal or vegetable matter, or a mixture of both. When the ſmell emitted during the incineration is ſimilar to that of burnt feathers, it is a certain indication of ſome animal matter; and a copious blue flame at the time of ignition, almoſt always denotes a conſiderable proportion of vegetable matter. In caſes when the experiment is needed to be very quickly performed, the deſtruction of the decompoſible ſubſtances may be aſſiſted by the agency of nitrate of ammoniac, which at the time of ignition may be thrown gradually upon the heated maſs in the quantity of twenty grains for every hundred of reſidual ſoil. It affords the principle neceſſary to the combuſtion of the animal and vegetable matter, which it cauſes to be converted into elaſtic fluids; and it is itſelf at the ſame time decompoſed and loſt.

The ſmell ſhews whether it be animal or vegetable.

XI. *Mode of ſeparating aluminous and ſilicious Matter and Oxide of Iron.*

The ſubſtances remaining after the decompoſition of the vegetable and animal matter, are generally minute particles of earthy matter, containing uſually alumine and ſilex with combined oxide of iron.

The reſidual ſilex, alumine and oxide of iron ſeparated.

To ſeparate theſe from each other, the ſolid matter ſhould be boiled for two or three hours with ſulphuric acid, diluted with four times its weight of water; the quantity of the acid ſhould be regulated by the quantity of ſolid reſiduum to be acted on, allowing for every hundred grains two drachms or one hundred and twenty grains of acid.

Dilute ſulphuric acid takes up the two firſt.

The

The ſubſtance remaining after the action of the acid, may be conſidered as ſilicious; and it muſt be ſeparated and its weight aſcertained, after waſhing and drying in the uſual manner.

Carbonate of ammonia throws down the alumine.

The alumine and the oxide of iron, if they exiſt, are both diſſolved by the ſulphuric acid; they may be ſeparated by carbonate of ammoniac, added to exceſs; it throws down the alumine, and leaves the oxide of iron in ſolution, and this ſubſtance may be ſeparated from the liquid by boiling.

Should any magneſia and lime have eſcaped ſolution in the muriatic acid, they will be found in the ſulphuric acid; this, however, is ſcarcely ever the caſe; but the proceſs for detecting them and aſcertaining their quantities, is the ſame in both inſtances.

More accurate proceſs.

The method of analyſis by ſulphuric acid, is ſufficiently preciſe for all uſual experiments; but if very great accuracy be an object, dry carbonate of potaſh muſt be employed as the agent, and the reſiduum of the incineration muſt be heated red for half an hour, with four times its weight of this ſubſtance, in a crucible of ſilver, or of well baked porcelain. The maſs obtained muſt be diſſolved in muriatic acid, and the ſolution evaporated till it is nearly ſolid; diſtilled water muſt then be added, by which the oxide of iron and all the earths, except ſilex, will be diſſolved in combination as muriates. The ſilex, after the uſual proceſs of lixiviation, muſt be heated red; the other ſubſtances may be ſeparated in the ſame manner as from the muriatic and ſulphuric ſolutions.

This proceſs is the one uſually employed by chemical philoſophers for the analyſis of ſtones.

XII. *Mode of diſcovering ſoluble Animal and Vegetable Matter, and Saline Matter.*

Matters ſoluble in water.

If any ſaline matter, or ſoluble vegetable or animal matter, is ſuſpected in the ſoil, it will be found in the water of lixiviation uſed for ſeparating the ſand.

This water muſt be evaporated to dryneſs in an appropriate diſh, at a heat below its boiling point.

If the ſolid matter obtained is of a brown colour and inflammable, it may be conſidered as partly vegetable extract. If its ſmell, when expoſed to heat, be ſtrong and fetid, it contains animal mucilaginous or gelatinous ſubſtance; if it be white

white and tranſparent, it may be conſidered as principally ſaline matter. Nitrate of potaſh (nitre) or nitrate of lime, is indicated in this ſaline matter, by its ſcintillating with a burning coal. Sulphate of magneſia may be detected by its bitter taſte; and ſulphate of potaſh produces no alteration in ſolution of carbonate of ammoniac, but precipitates ſolution of muriate of barytes.

XIII. *Mode of detecting Sulphate of Lime (Gypſum) and Phoſphate of Lime in Soils.*

Sulphate of Lime.

Should ſulphate of phoſphate of lime be ſuſpected in the entire ſoil, the detection of them requires a particular proceſs upon it. A given weight of it, for inſtance four hundred grains, muſt be heated red for half an hour in a crucible, mixed with one-third of powdered charcoal. The mixture muſt be boiled for a quarter of an hour, in a half pint of water, and the fluid collected through the filter, and expoſed for ſome days to the atmoſphere in an open veſſel. If any ſoluble quantity of ſulphate of lime (gypſum) exiſted in the ſoil, a white precipitate will gradually form in the fluid, and the weight of it will indicate the proportion.

Phoſphate of lime.

Phoſphate of lime, if any exiſt, may be ſeparated from the ſoil after the proceſs for gypſum. Muriatic acid muſt be digeſted upon the ſoil, in quantity more than ſufficient to ſaturate the ſoluble earths; the ſolution muſt be evaporated, and water poured upon the ſolid matter. This fluid will diſſolve the compounds of earths with the muriatic acid, and leave the phoſphate of lime untouched.

It would not fall within the limits aſſigned to this paper, to detail any proceſſes for the detection of ſubſtances which may be accidentally mixed with the matters of ſoils. Manganeſe is now and then found in them, and compounds of the barytic earth; but theſe bodies appear to bear little relation to fertility or barrenneſs, and the ſearch for them would make the analyſis much more complicated without rendering it more uſeful.

XIV. *Statement of Reſults and Products.*

Products ſtated.

When the examination of a ſoil is compleated, the products ſhould be claſſed, and their quantities added together, and if they nearly equal the original quantity of ſoil, the analyſis may be conſidered as accurate. It muſt, however, be noticed,

that when phoſphate or ſulphate of lime are diſcovered by the independent proceſs XIII. a correction muſt be made for the general proceſs, by ſubtracting a ſum equal to their weight from the quantity of carbonate of lime, obtained by precipitation from the muriatic acid.

In arranging the products, the form ſhould be in the order of the experiments by which they were obtained.

Thus 400 grains of a good ſilicious ſandy ſoil may be ſuppoſed to contain

	Grains.
Of water of abſorption	18
Of looſe ſtones and gravel principally ſilicious	42
Of undecompounded vegetable fibres	10
Of fine ſilicious ſand	200
Of minutely divided matter ſeparated by filtration and conſiſting of	
Carbonate of lime	25
Carbonate of magneſia	4
Matter deſtructible by heat, principally vegetable	10
Silex	40
Alumine	32
Oxide of iron	4
Soluble matter, principally ſulphate of potaſh and vegetable extract	5
Gypſum	3
Phoſphate of lime	2
	125
Amount of all the products	395
Loſs	5

In this inſtance the loſs is ſuppoſed ſmall; but in general, in actual experiments, it will be found much greater, in conſequence of the difficulty of collecting the whole quantities of the different precipitates; and when it is within thirty for four hundred grains, there is no reaſon to ſuſpect any want of due preciſion in the proceſſes.

XV. *This general Method of Analyſis may in many Caſes be much ſimplified.*

Simplification, &c. of the analyſis.

When the experimenter is become acquainted with the uſe of the different inſtruments, the properties of the reagents, and

and the relations between the external and chemical qualities of ſoils, he will ſeldom find it neceſſary to perform, in any one caſe, all the proceſſes that have been deſcribed. When his ſoil, for inſtance, contains no notable proportion of calcareous matter, the action of the muriatic acid IX. may be omitted. In examining peat ſoils, he will principally have to attend to the operation by fire and air X.; and in the analyſis of chalks and loams, he will often be able to omit the experiment by ſulphuric acid XI.

In the firſt trials that are made by perſons unacquainted with chemiſtry, they muſt not expect much preciſion of reſult. Many difficulties will be met with; but in overcoming them, the moſt uſeful kind of practical knowledge will be obtained; and nothing is ſo inſtructive in experimental ſcience, as the detection of miſtakes. The correct analyſt ought to be well grounded in chemical information; but perhaps there is no better mode of gaining it, than that of attempting original inveſtigations. In purſuing his experiments, he will be continually obliged to learn from books, the hiſtory of the ſubſtances he is employing or acting upon; and his theoretical ideas will be more valuable in being connected with practical operation, and acquired for the purpoſe of diſcovery.

XVI. *On the Improvement of Soils, as connected with the Principle of their Compoſition.*

Improvement of lands from the known compoſition of fertile or ſterile ſoils.

In caſes when a barren ſoil is examined with a view to its improvement, it ought in all caſes, if poſſible, to be compared with an extremely fertile ſoil in the ſame neighbourhood, and in a ſimilar ſituation: the difference given by their analyſes would indicate the methods of cultivation; and thus the plan of improvement would be founded upon accurate ſcientific principles.

If the fertile ſoil contained a large quantity of ſand, in proportion to the barren ſoil, the proceſs of amelioration would depend ſimply upon a ſupply of this ſubſtance; and the method would be equally ſimple with regard to ſoils deficient in clay or calcareous matter.

In the application of clay, ſand, loam, marle, or chalk to lands, there are no particular chemical principles to be obſerved; but when quick lime is uſed, great care muſt be taken that it is not obtained from the magneſian limeſtone; for in this

this caſe, as has been ſhewn by Mr. Tennant, it is exceedingly injurious to land *. The magneſian limeſtone may be diſtinguiſhed from the common limeſtone by its greater hardneſs, and by the length of time that it requires for its ſolution in acids, and it may be analyſed by the proceſs for carbonate of lime and magneſia IX.

When the analytical compariſon indicates an exceſs of vegetable matter, as the cauſe of ſterility, it may be deſtroyed by much pulverization and expoſure to air, by paring and burning, or the agency of lately made quicklime. And the defect of animal and vegetable matter muſt be ſupplied by animal or vegetable manure.

XVII. *Sterile Soils in different Climates and Situations muſt differ in Compoſition.*

Different climates and local circumſtances require different compounds for fertile ſoils.

The general indications of fertility and barrenneſs, as found by chemical experiments, muſt neceſſarily differ in different climates, and under different circumſtances. The power of ſoils to abſorb moiſture, a principle eſſential to their productiveneſs, ought to be much greater in warm and dry countries, than in cold and moiſt ones; and the quantity of fine aluminous earth they contain larger. Soils likewiſe that are ſituated on declivities, ought to be more abſorbent than thoſe in the ſame climate on plains or in valleys †. The productiveneſs of ſoils muſt likewiſe be influenced by the nature of the ſubſoil, or the earthy or ſtony ſtrata on which they reſt; and this circumſtance ought to be particularly attended to, in conſidering their chemical nature, and the ſyſtem of improvement. Thus a ſandy ſoil may ſometimes owe its fertility to the power of the ſubſoil to retain water; and an abſorbent clayey ſoil may occaſionally be prevented from being barren, in a moiſt climate, by the influence of a ſubſtratum of ſand or gravel.

XVIII. *Of the chemical Compoſition of fertile Corn Soils in the Climate.*

Actual compoſition of ſome fertile ſoils.

Thoſe ſoils that are moſt productive of corn, contain always certain proportions of aluminous and calcareous earth in a finely divided ſtate, and a certain quantity of vegetable or animal matter.

* Phil. Tranſactions for 1799, p. 305. This limeſtone is found abundantly in Yorkſhire, Derbyſhire, and Somerſetſhire.

† Kirwan. Tranſ. Iriſh Academy, Vol. V. p. 175.

The

The quantity of calcareous earth is however very various, and in ſome caſes exceedingly ſmall. A very fertile corn ſoil from Ormiſton in Eaſt Lothian afforded me in an hundred parts, only eleven parts of mild calcareous earth; it contained twenty-five parts of ſilicious ſand; the finely divided clay amounted to forty-five parts. It loſt nine in decompoſed animal and vegetable matter, and four in water, and afforded indications of a ſmall quantity of phoſphate of lime.

This ſoil was of a very fine texture, and contained very few ſtones or vegetable fibres. It is not unlikely that its fertility was in ſome meaſure connected with the phoſphate; for this ſubſtance is found in wheat, oats, and barley, and may be a part of their food.

A ſoil from the low lands of Somerſetſhire, celebrated for producing excellent crops of wheat and beans without manure, I found to conſiſt of one-ninth of ſand, chiefly ſilicious, and eight-ninths of calcareous marle tinged with iron, and containing about five parts in the hundred of vegetable matter. I could not detect in it any phoſphate or ſulphate of lime, ſo that its fertility muſt have depended principally upon its power of attracting principles of vegetable nouriſhment from water and the atmoſphere *.

Mr. Tillet, in ſome experiments made on the compoſition of ſoils at Paris, found that a ſoil compoſed of three-eighths of clay, two-eighths of river ſand, and three-eighths of the parings of limeſtone, was very proper for wheat.

XIX. *Of the Compoſition of Soils proper for bulbous Roots and for Trees.*

Soils proper for bulbous roots and trees.

In general, bulbous roots require a ſoil much more ſandy, and leſs abſorbent than the graſſes. A very good potatoe ſoil, from Varfel in Cornwall, afforded me ſeven-eighths of ſilicious ſand; and its abſorbent power was ſo ſmall, that one hundred parts loſt only two by drying at 400 Fahrenheit.

Plants and trees, the roots of which are fibrous and hard, and capable of penetrating deep into the earth, will vegetate to advantage in almoſt all common ſoils, which are moderately dry, and which do not contain a very great exceſs of vegetable matter.

* This ſoil was ſent to me by T. Pooie, Eſq. of Nether Stowey. It is near the opening of the river Parret into the Britiſh Channel; but, I am told, is never overflowed.

I found

I found the ſoil taken from a field at Sheffield-place in Suſſex, remarkable for producing flouriſhing oaks, to conſiſt of ſix parts of ſand, and one part of clay and finely divided matter. And one hundred parts of the entire ſoil ſubmitted to analyſis, produced

	Parts.
Water	3
Silex	54
Alumine	28
Carbonate of lime	3
Oxide of iron	5
Decompoſing vegetable matter	4
Loſs	3

XX. *Advantages of Improvements made by changing the Compoſition of the earthy Parts of Soils.*

Soils rendered fertile by changing the compoſition of the earthy parts, are more permanent than manured ſoils.

From the great difference of the cauſes that influence the productiveneſs of lands, it is obvious that in the preſent ſtate of ſcience, no certain ſyſtem can be deviſed from their improvement, independent of experiment; but there are few caſes in which the labour of analytical trials will not be amply repaid by the certainty with which they denote the beſt methods of amelioration; and this will particularly happen, when the defect of compoſition is found in the proportions of the primitive earths.

In ſupplying animal or vegetable manure, a temporary food only is provided for plants, which is in all caſes exhauſted by means of a certain number of crops; but when a ſoil is rendered of the beſt poſſible conſtitution and texture, with regard to its earthy parts, its fertility may be conſidered as permanently eſtabliſhed. It becomes capable of attracting a very large portion of vegetable nouriſhment from the atmoſphere, and of producing its crops with comparatively little labour and expence.

Deſcription of the Apparatus for the Analyſis of Soils.

Apparatus for experiments.

A. Retort.

B, B. Funnels for the purpoſe of filtrating.

D. Balance.

E. Argand's lamp.

F, G, H, K. The different parts of the apparatus required for meaſuring the quantity of elaſtic fluid given out during the

the action of an acid on calcareous ſoils. F. Repreſents the bottle for containing the ſoil. K. The bottle containing the acid furniſhed with a ſtopcock. G. The tube connected with a flaccid bladder. I. The graduated meaſure. H. The bottle for containing the bladder. When this inſtrument is uſed, a given quantity of ſoil is introduced into F; K is filled with muriatic acid diluted with an equal quantity of water; and the ſtop-cock being cloſed is connected with the upper orifice of F, which is ground to receive it. The tube G is introduced into the lower orifice of F, and the bladder connected with it placed in its flaccid ſtate into H, which is filled with water. The graduated meaſure is placed under the tube of H. When the ſtop-cock of K is turned, the acid flows into F, and acts upon the ſoil; the elaſtic fluid generated paſſes through G into the bladder, and diſplaces a quantity of water in H equal to it in bulk, and this water flows through the tube into the graduated meaſure; the water in which gives by its volume the indication of the proportion of carbonic acid diſengaged from the ſoil; for every ounce meaſure of which two grains of carbonate of lime may be eſtimated.

L. Repreſents the ſtand for the lamp.

M, N, O, P, Q, R, S. Repreſent the bottles containing the different reagents.

IV.

Diſcovery of a new Vegetable Subſtance, by Mr. Rose *.

A CONCENTRATED decoction of the root of elecampane, *inula helenium*, after ſtanding ſome hours, depoſits a white powder, appearing at firſt ſight much like ſtarch, but differing from it both in its principles and in its manner of comporting itſelf with other ſubſtances.

Decoction of elecampane root depoſits a powder reſembling ſtarch.

1. This ſubſtance is generally inſoluble in cold water. Being triturated with it a white milky liquor is formed, which ſoon depoſits a heavy white powder, and leaves the ſupernatant water clear and limpid.

It is inſoluble in cold water.

2. It diſſolves very well in boiling water. On heating to ebullition one part of the white powder, with four parts of

Soluble in boiling water.

* From Gehlen's Journal for 1804, Vol. III. p. 217.

water, a complete ſolution is obtained, which paſſes through filtering paper while hot, but on cooling acquires a mucilaginous conſiſtence and a dull colour. In the courſe of ſome hours this ſolution depoſits the greater part of the ſubſtance diſſolved in the form of a compact white powder.

But much ſubſides on cooling.

Differs from ſolution of gum-arabic.

A ſolution of one part of gum arabic, in four parts of water is much thicker, of a more tenacious conſiſtence, and froths lightly, which the ſolution of the powder from the elecampane root does not.

Alcohol ſeparates it from water,

does not gum-arabic.

3. On mixing the ſolution of the white powder with an equal quantity of alcohol, the mixture is at firſt clear, but in a little time the powder ſeparates in the form of a tumid white ſediment, leaving the fluid above it tranſparent. A ſolution of gum-arabic on the addition of alcohol becomes immediately milky, and long retains this appearance, no kind of powder ſeparating even in ſeveral days.

Melts, emits a thick ſmoke, and leaves little reſiduum.

Thus differs from ſtarch,

and from gum.

4. When thrown on burning coals, the white powder melts like ſugar and evaporates, diffuſing a white, thick, pungent ſmoke, with a ſmell of burnt ſugar. After this combuſtion a light reſiduum only remains, which runs into the coal. Starch emits a ſimilar ſmoke, but does not melt, and leaves a coally reſiduum much greater in quantity. Gum-arabic under the ſame circumſtances gives out ſcarcely any ſmoke.

On red hot iron burns.

Starch.

Gum.

Heated in an iron ſpoon over charcoal the powder firſt melts, and emits the ſmoke above deſcribed. As ſoon as the ſpoon becomes red hot, it burns with a vivid light flame, and leaves a very trifling coally reſiduum. Starch under the ſame circumſtances does not melt, is much longer before it burns, and leaves a conſiderable reſiduum of coally matter. Gum-arabic only ſparkles, does not take fire, and leaves a great deal of coal, which is readily convertible into grayiſh aſhes.

Dry diſtillation produces an acid, but no oil.

5. By dry diſtillation we obtain from this powder of the elecampane root a brown empyreumatic acid, having the ſmell of pyroxalic acid, but not an atom of empyreumatic oil.

Nitrid acid produces malic, oxalic, and in exceſs acetic.

Gum the ſaccholactic acid.

Starch fat.

6. The nitric acid transforms the powder only into malic acid and oxalic acid, and when uſed in great exceſs into acetic acid: but we do not obtain an atom of the ſaccholactic acid, which gum-arabic treated in the ſame manner furniſhes ſo abundantly; or of the fatty matter which is generated by the action of nitric acid on ſtarch.

From all these phenomena it follows, that this farinaceous powder extracted from elecampane root, is neither starch nor gum, but a peculiar vegetable substance holding a middle rank between the two. It is probable, that it exists in many other vegetables, and that several products hitherto considered as starch are of the same nature as this farina.

Hence of a nature between gum and starch, and probably exists in other vegetables.

V.

New Galvanic Discoveries by Mr. RITTER, *extracted from a Letter from Mr.* CHRIST. BERNOULLI *.

I HERE transmit you the information you requested respecting the late experiments of Mr. Ritter, to which I subjoin some account of that gentleman.

1. *Charging of a Louis d'Or by the Pile.*

The pile with which Mr. Ritter usually makes his experiments consists of a hundred pairs of metallic plates, two inches in diameter. The pieces of zinc have a rim to prevent the liquid pressed out from flowing away. The apparatus is always insulated by several plates of glass.

Mr. Ritter's common pile.

As Mr. Ritter at present resides in a village near Jene, I have not been able to see his experiments with his grand battery of two thousand pieces, or with his battery of fifty pieces, each thirty-six inches square, the action of which continues very perceptible for a fortnight. Neither have I seen his experiments with the new battery of his invention, consisting of a single metal, and which he calls *the charging pile*.

His grand battery, large battery, and charging pile.

I have frequently however, seen him galvanise louis d'or lent him by persons present. To effect this, he places the louis between two pieces of pasteboard thoroughly wetted, and keeps it six or eight minutes in the chain of circulation connected with the pile. Thus the louis becomes charged, without being immediately in contact with the conducting wires. If this louis be applied afterward to the crural nerves of a frog recently prepared, the usual contractions will be

Louis d'or charged by being kept in the galvanic circuit, excites contractions,

* Translated from the *Journal de Chimie and de Physique* of Van Mons, No. 17, p. 133, March, 1805.

and may thus be diſtinguiſhed among others,

excited. I had put a louis thus galvaniſed into my pocket, and Mr. Ritter ſaid to me a few minutes after, that I might find out this louis from among the reſt, by trying them in ſucceſſion upon the frog. Accordingly I made the trial, and in reality diſtinguiſhed among ſeveral others a ſingle one, in which the exciting quality was very evident. This charge is retained in proportion to the time that the piece has remained in the circuit of the pile. Of three different louis which Mr. Ritter charged in my preſence, neither loſt its charge in leſs than five minutes. All theſe experiments ſucceeded completely, and nothing ſeemed ſo eaſy as to repeat them.

as it does not loſe its charge for ſome minutes.

This ſhows the affinity of the galvanic with the magnetic fluid, between which and the electric, it holds a middle place.

This retention of the galvanic charge by a metal in contact with the hand, and with other metals, ſhews this communication of the galvanic virtue to have more affinity with magnetiſm than with electricity, and aſſigns to the galvanic fluid an intermediate rank between the other two.

Several pieces may be charged at once.

In the manner which I have juſt deſcribed, Mr. Ritter can charge at once as many pieces as he wiſhes. It is ſufficient if the two extreme pieces of the number communicate with the pile through the intervention of wet paſteboards. It is with metallic diſcs charged in this manner, and placed upon one another with pieces of wet paſteboard alternately interpoſed, that Mr. Ritter conſtructs his charging pile, which ought in remembrance of its inventor to be called the *Ritterian pile*. The conſtruction of this pile ſhows, that each metal galvaniſed in this way acquires polarity, as the needle does when touched with a magnet. Though I have had no opportunity of ſeeing this new pile, I have convinced myſelf of the reality of the phenomenon by an experiment of the higheſt importance to ſcience, and for the invention of which we are equally indebted to the ſame ingenious philoſopher.

Ritterian pile.

Metals thus charged acquire polarity.

2. *Different Excitability of the Parts of Animals.*

Different excitability of the parts of animals.

During the courſe of ſeveral years in which Mr. Ritter has been employed in galvanic purſuits, and during which he has made many thouſands of experiments on the excitation produced in the frog by the contact of two different metals, for Mr. Ritter has not entirely abandoned the original mode of galvaniſing, like moſt other experimentaliſts, who employ Volta's pile excluſively; he had perceived not only a very ſtriking difference in the excitability of the different parts of animals,

animals, but alſo a difference of excitement between the extenſor and flexor muſcles, according as the poſitive or negative pole was applied to them, or as they were acted upon the inſtant after the metals were brought into contact or ſeparated from each other.

When the excitability is at its higheſt point of energy, as in very young frogs the moment after they are prepared, or in adult frogs during the coupling ſeaſon, the flexors alone contract, and in particular the flexor muſcles of that thigh to which the ſilver or negative metal is applied, contract at the inſtant when the metals come into contact, while thoſe of the thigh to which the zinc or poſitive metal is applied, contract at the inſtant of their ſeparation.

When the excitability of the animal is greateſt, the flexors contract by poſitive galvaniſm:

The oppoſite effects are obſervable in frogs, the excitability of which is on the point of being extinguiſhed, (Ritter's fifth degree.) In this caſe the extenſors only contract, and the flexors remain abſolutely motionleſs. At the moment of contact of the metals the muſcles on the zinc ſide alone are thrown into action, and at the moment of ſeparation thoſe on the ſilver ſide.

when it is loweſt the extenſors contract by negative.

Mr. Ritter diſtinguiſhes three degrees of mean excitability. At the ſecond degree (the firſt of the three mean degrees,) when the metals are brought into contact, a ſtrong excitement of the flexors is diſplayed on the ſilver ſide, and a weak excitement of the extenſors on the zinc ſide; and when the metals are ſeparated a ſtrong excitement of the flexors is ſeen on the zinc ſide, and a weak excitement of the extenſors on the ſilver ſide.

When the eccitability is between the medium and either extreme, the effect on the flexors and extenſors ſimultaneous but unequal.

At the fourth degree of excitability the contrary takes place. At the third or middle degree the excitability appears to be equally diſtributed, the contractions on each ſide appear equal, and at the moment of contact the flexors contract on the ſilver ſide, the extenſors on the zinc ſide; while at the moment of ſeparation the extenſors contract on the ſilver ſide, and the flexors on the zinc ſide.

At the medium, equal as well as ſimultaneous.

Mr. Ritter ſhowed me all theſe phenomena, and it was very eaſy to diſtinguiſh the different contractions. I have not yet had time to repeat theſe experiments, but I am afraid, eaſy as they appeared to be, they will require an experienced hand, to produce ſuch diſtinct effects as I ſaw. None of the experiments which Mr. Ritter performed before me ſucceeded with him

The experiments do not always ſucceed on the firſt trial.

Mr. Ritter's merit not ſufficiently appreciated,

him the firſt time. Moſt of theſe experiments have never yet been made public, and few philoſophers have juſtly appreciated the value of thoſe which have been given to the world. There are ſome people, who, habituated ſolely to the ſtriking effects of groſſer phyſics, ſuppoſe it impoſſible for a young philoſopher to ſee any thing more than themſelves in the delicate phenomena of a more refined order of phyſical experiments. What has greatly contributed to prevent Mr. Ritter from attaining the high reputation he deſerves is his ſtyle, which, by endeavouring to give it preciſion, he has rendered obſcure; but in converſation it is quite otherwiſe, as here he combines the ſtricteſt logic with the greateſt ſimplicity of expreſſion.

partly owing to his ſtyle.

Account of Mr. Ritter.

Mr. Ritter is one of thoſe men, who owe every thing to the inſpiration of genius, nothing to education. He was intended for a mechanical occupation, when the diſcoveries of galvani excited in him that innate taſte for the phyſical ſciences, which has carried him over every obſtacle, and raiſed him to rank among the firſt natural philoſophers. Deſtitute of every ſource for procuring himſelf the apparatus indiſpenſable to ordinary phyſics, but ſwayed by the enthuſiaſm of inquiry, he greedily ſeized the opportunity of obeying this impulſe by purſuing a ſeries of experiments, that require only a ſimple and not a very expenſive apparatus. Europe has rung with the ſucceſs he has obtained within the ſeven years he has given to his reſearches. He muſt have written much to procure himſelf a large pile, and the moſt neceſſary books of natural philoſophy.

Not leſs indefatigable as an experimenter than ingenious as a theoriſt, he has committed to writing thouſands of experiments, which his time divided between galvanic experiments, application to other branches of phyſics, and the ſtudy of languages, has not yet allowed him to put in order for publication. But this ſtate of conſtraint is about to be at an end. The elector Bavaria, that enlightened prince, whoſe philoſophical beneficence attracts to his dominions the moſt diſtinguiſhed men of ſcience and learning throughout Europe, has juſt appointed Mr. Ritter a Member of the Academy of Munich, with a ſalary of about 200l. a year.

He is compoſing a ſyſtematic work on galvaniſm.

Mr. Ritter has been employed theſe ſix months in compoſing a ſyſtematic work on galvaniſm, but he does not think he

he ſhall be able to finiſh it in leſs than two or three years. When I left him he was going to publiſh *Tables of Galvanic Affinity*, including all the ſubſtances on which he has made experiments. Theſe tables will be of as much importance to galvaniſm as thoſe of Bergman were to chemiſtry: they will ſhow, though not yet in a complete manner, the order in which ſubſtances follow each other with reſpect to exciting or receiving the galvanic action.

Publiſhing tables of galvanic affinity.

But to return to the experiments reſpecting the charging of metals. Mr. Ritter, after having ſhown me his experiments on the different contractibility of various muſcles, made me obſerve, that the piece of gold galvaniſed by communication exerts at once the action of two metals, or of one conſtituent part of the pile; and that the half which was next the negative pole while in the circle became poſitive, and the half toward the poſitive pole became negative. I was completely convinced of the reality of theſe different phenomena, ſo important to phyſic in general, and to phyſiology in particular.

The galvaniſed piece of metal has two poles.

Mr. Ritter having diſcovered the method of galvaniſing metals, as iron is rendered magnetic, and having obſerved that galvaniſed metals always exhibit two poles, as the magnetic needle does, had the curioſity to obſerve the effect of golden needles charged with galvaniſm and balanced on a pivot. To his ſurpriſe he perceived, that theſe needles had a certain dip and variation, and that the angle of variation, the quantity of which I am ſorry I cannot recollect, was uniformly the ſame in all his experiments. It differs however from that of the magnetic needle, and the poſitive pole always dips.

Golden needles galvaniſed and ſuſpended, have both dip and variation, but different from the magnetic.

VI.

Improvement in applying the Points in Electrical Machines. By Mr. G. J. Singer.

To Mr. NICHOLSON.

SIR, *Princes Street, Sep. 19th,* 1805.

In the ordinary conſtruction of electrical machines, the collecting points are fixed, and by the leaſt accidental motion are liable to ſcratch the glaſs, to obviate this inconvenience, I place

place my points in a cylindrical wire, terminated by ſmooth wooden balls, whoſe ſemidiameter is leſs than the length of the points: This wire is moveable on its axis, by means of a ſpring ſocket annexed to the ſtem which enters the conductor: The points may of courſe be placed at any required elevation, and the greateſt intenſity any variation in their ſituation produces, be obtained. When the points are elevated a little above the horizontal line, the danger of ſcratching the glaſs is effectually prevented, by the balls coming in contact while the points are kept at a ſmall diſtance. The ſecurity this application produces, and the additional intenſity it affords, have induced me to trouble you with this communication.

I am,

Dear Sir,

Your's, &c.

G. J. SINGER.

VII.

Queſtion whether Light as a Body may not have its Temperature raiſed or lowered, and produce the Effects aſcribed to reflected Heat. By J. P.

To Mr. NICHOLSON.

SIR,

Queſtion reſpecting light.

POSSESSING no differential thermometer, nor any time to employ it, I cannot prove whether my opinion is well founded or not, reſpecting the ingenious experiments of Mr. Leſlie or of M. Pictet, by which the reflection of inviſible (not radiant) heat, and even of cold, appears to have been proved.

Inſtead of there being an actual reflection of heat as a ſubſtance, or of cold as a ſubſtance, is it not in all theſe caſes a reflection of heated or of cooled light? In the experiments with the heated canniſter, the light of the room is, I doubt not, heated by the canniſter; and if collected in a focus, muſt produce an effect on the thermometer, anſwerable to the increaſed quantity of heat with which it is impregnated. Thus alſo in Monſ. Pictet's experiment, the light intercepted by the mirror and thence reflected, has been deprived of a portion of its caloric, or in other words cooled, by the ice; at the

focal

focal point therefore will be a collection of cooled rays of light, which must necessarily occasion an effect on the thermometer, the reverse of that of the former experiment. That light is a body capable of being united with caloric, and that heated or cooled light should thus be reflected and occasion all the phenomena of Mr. Leslie's and of M. Pictet's experiments, appears to be much more probable, than that this calorific and frigorific fluid should be the ambient air, or that cold, as a body, should be reflected from mirrors in such a manner as light is perhaps only capable of being reflected. Were the experiments so made that no light should be in the room, and only a small confined portion of light used to examine the thermometer, these conjectures would be put to the trial, and I trust the mystery would be removed.

Sir, your's,

J. P.

VIII.

*Experiments on a Mineral formerly called false Tungstein, now Cerite, in which a new Metal has been found.**

MR. Klaproth, about eight months ago, says Mr. Vauquelin, sent me word, that he had discovered in the tungstein of Bastnas a new earth, to which he gave the name of ochroit, on account of the red colour it acquired by calcination. Messrs. Hisinger and Berzelius, hearing this, wrote to Mr. V. claiming the priority of discovery, but affirming at the same time, that what they had found was a new metal. These gentlemen afterward sent Mr. V. specimens of the mineral, which he analysed in company with two experienced practical chemists, Messrs. Tassaert and Bergman. The following were the results of their analysis:

Klaproth supposed he had discovered a new earth in the false tungstein;

Hisinger and Berzelius a new metal.

The pure cerite † is semitransparent, with a slight rosy tinge,

Characters of pure cerite.

* Abridged from a paper by Vauquelin in the *Annales de Chimie*, Vol. LIV. p. 28, and another by Messrs. Hisinger and Berzelius in van Mons's *Journal de Chimie*, Vol. VI. p. 142.—C.

† Messrs. H. and B. have given to the metal the name of *cerion* or *cerium*, from the new planet Ceres, and to the mineral in which they discovered it that of *cerite*.

or of a light or deep fleſh-colour *. It is ſufficiently hard to ſcratch glaſs †, ſtrikes fire with difficulty, and its ſpecific gravity is 4·530. It has no determinate cryſtalline figure. Its fracture is compact ‡, and a little ſhining. Its powder is of a greyiſh colour; it becomes yellow by calcination, and loſes twelve per cent §.

Treated with nitro-muriatic acid.

Exp. 1. Two hundred parts of this mineral treated with nitro-muriatic acid three times ſucceſſively, gave abundance of nitrous acid and oxigenated muriatic acid gas. The firſt and ſecond ſolutions being diluted with water were of a gold colour; the third was colourleſs. The former two being mixed depoſited ſpontaneouſly in time a ſmall quantity of white ſediment. The reſiduum left by the nitro-muriatic acid was of a gray colour with a ſlight roſeate tinge, and weighed 62, ſo that 138 parts were diſſolved.

The ſolution precipitated

Exp. 2. The ſolutions being evaporated to the conſiſtence of ſyrup to volatiliſe, the ſuperfluous acid remained clear to the end of the operation. Their reſiduum, diluted with water, afforded a milky liquor, with a ſlight roſy tint, and a very aſtringent taſte.

by pruſſiate of potaſh and ammonia.

Pruſſiate of potaſh produced in it a greeniſh blue precipitate: the colour of which was changed to a brown by a ſmall quantity of ammonia.

All the liquor into which a ſmall quantity of ammonia had been put to precipitate the iron alone was poured into a filter, but would not paſs through. It was heated therefore, and filtered, when it appeared of a gold colour, and had a very ſaccharine taſte. Pruſſiate of potaſh and oxalate of ammonia threw down from it perfectly white precipitates.

The matter left on the filter continued for a long time to impart a yellow tinge to the water with which it was waſhed. It was of a red colour, and appeared like oxide of iron at a maximum of oxidation.

Examined by different reagents.

The ſolution thus deprived of the red matter by ammonia, was examined by various reagents. Pruſſiate of potaſh gave with it a white, flocculent, gelatinous precipitate. Infuſion

* Opake, and ſometimes but very rarely, yellowiſh. Meſſrs. H. and B.

† Does not ſcratch glaſs. H. and B.

‡ Unequal and angular. H. and B.

§ Six or ſeven. H. and B.

of

of galls, a brown, flocculent ſediment, unaffected by muriatic acid. Carbonate of potaſh, a very copious white gelatinous precipitate. Cauſtic potaſh, the ſame; and an exceſs of this reagent produced no change. Oxalate of ammonia, a very copious, white, flocculent precipitate, inſoluble in an exceſs of oxalic acid. Sulphuric acid, a yellow cryſtalline precipitate ſoluble in water. Muriate of tin whitened the ſolution without forming any precipitate.

Attempts to free it from iron.

Exp. 3. After this the ſolution was evaporated, when it inſtantly became turbid, and formed an abundant fleſh-coloured depoſit. This was treated with acidulous oxalate of potaſh to diſſolve the iron without ſucceſs: the addition of nitric acid was as unſucceſsful: but muriatic acid added to the preceding diſſolved the precipitate with effervеſcence and the emiſſion of oxigenated muriatic acid gas. A white cryſtalline ſubſtance however, remained, conſiſting of oxide of cerium with oxalic acid. The greater part of the exceſs of acid in the ſolution being ſaturated with ammonia, oxalate of ammonia was added till no more precipitate was formed. This precipitate had all the properties of oxalate of cerium. Ammonia threw down from the filtered liquor oxide of iron.

Muriate precipitated by oxalate of ammonia.

Exp. 4. The matter precipitated from the ſolution of cerium by ammonia in *Exp.* 2, diſſolved with effervеſcence in muriatic acid. Oxalate of ammonia threw down from this ſolution oxide of cerium, and the filtered liquor contained oxide of iron tolerably pure.

Nitrate precipitated by oxalate of ammonia.

Exp. 5. The liquor freed from the greater part of the iron by ammonia and heat, which had notwithſtanding a ſlight roſeate tinge, was precipitated by oxalate of ammonia. The precipitate at the moment of its formation had the appearance of muriate of ſilver, but ſoon became granulous and ſubſided in this form. The liquor paſſed through the filter colourleſs, and the roſy tinge remained in the oxalate.

Oxalate of lime ſeparated.

Exp. 6. As the liquor from which the oxalate of cerium was precipitated contained an exceſs of acid, it might be preſumed to hold in ſolution moſt of the oxalate of lime formed at the ſame time, if the cerite contained any. Accordingly it was mixed with the water that had waſhed the precipitate and concentrated by evaporation, when on the addition of ammonia a ſmall quantity of oxalate of lime was thrown down.

Reſt of the lime ſeparated.

Exp. 7. As notwithſtanding ſome oxalate of lime might have been precipitated with the oxalate of cerium, a portion of the red

red oxide of cerium arifing from the decompofition of the oxalate by calcination was diffolved in muriatic acid. A brifk effervefcence inftantly took place, with the evolution of oxigenated muriatic gas, which continued till the whole was diffolved, and differed in no refpect from that prepared with oxide of manganefe.

Muriatic folution of cerium rendered folid by ammonia.

The folution of cerium in muriatic acid was clear, and had only a light rofy tinge. To feparate it from the lime, if there were any, ammonia was added, when the folution, having been diluted with but a fmall quantity of water, congealed into a femitranfparent gelatinous mafs, which it was neceffary to agitate with a great deal of water, before it could be gotten out of the bottle.

The precipitate being waſhed and calcined was very compact, and had a brilliant fracture.

The liquor thus decompofed by ammonia contained lime, as appeared on precipitating it with oxalate of ammonia.

Oxalate of cerium.

At the inftant when the oxalate of cerium is precipitated by ammonia it is white and femitranfparent; but by agitation in the air and deficcation it aſſumes a yellowifh colour, and becomes opake. A remarkable circumftance is, that, if it be boiled with ammonia or potaſh before it is dry, it becomes again perfectly white and opake. This is not owing to any combination of the alcalies with the cerium, for when it has been well waſhed, no trace of them can be diſcovered by the moſt careful analyſis.

Does not combine with alkalies.

Component parts of cerite.

The reſiduum left untouched by the acids was afterwards examined; when it appeared, that the pureſt ore of cerium * from Baſtnas contained in 100 parts,

Oxide of cerium -	63
Silex - - -	17·5
Oxide of iron - -	2
Lime - - -	3 or 4
Water - -	12
	98·5 †

Cerium,

* Mr. Vauquelin analyſed other ſpecimens, which were mixed with green actinote and cupreous pyrites; but as nothing particular occurred in theſe analyſes, it is unneceſſary to enter into them.

† Meſſrs. H. and B. ſay: ſilex 23 parts, carbonate of lime 5·5, oxide of iron 22, and of oxide of cerium after calcination more than 50.

Cerium, like ſeveral other metals, appears ſuſceptible of two very diſtinct degrees of oxigenation: the oxide which contains leaſt oxigen is white; that which is ſaturated with it is of a fallow red. Though they differ conſiderably in certain reſpects, their quantities of oxigen are not very diſſimilar, whence they are readily and eaſily commutable into each other.

Cerium has two oxides.

The white oxide expoſed to the blowpipe ſoon becomes red, but does not melt, or even agglutinate. With a large proportion of borax it melts into a tranſparent yellow globule *: with leſs the globule becomes opake on cooling. On heating gently a tranſparent compound of borax and oxide of cerium it becomes milky like a tin enamel.

Expoſed to the blowpipe with borax.

The white oxide of cerium becomes yellowiſh in the open air, but never ſo red as by calcination, becauſe it readily combines with carbonic acid, which oppoſes its union with oxigen to the point of ſaturation, and becauſe it always retains a portion of water, which diminiſhes its colour.

Takes oxigen and carbonic acid from the air.

Cauſtic potaſh by the aſſiſtance of heat deprives the red oxide of part of its oxigen, and renders it white. This being dried, however, and urged to the ſtate of fuſion, becomes red again. Alcalies have no other action on it.

Alcalies do not act on it.

Sulphuric acid diſſolves the red oxide with great difficulty. Equal parts of it and of ſulphuric acid diluted with four times its weight of water combine readily when heated: the whole maſs aſſuming a cryſtalline form and brilliant aſpect. On adding freſh acid, and heating them together a long time, a complete ſolution takes place. This ſolution being evaporated by a gentle heat cryſtallizes in very ſmall needles, ſome of which are orange †, others of a lemon colour. If evaporated quickly, nothing but a yellow powder is obtained.

Sulphuric acid with the red oxide.

Two ſulphates.

50. The increaſe of weight they aſcribe to oxigen abſorbed by the iron and the cerium.

* Firſt blood-red, then, as the heat decreaſes, green, yellowiſh, and finally colourleſs. If it be kept in the middle of the flame it continues as clear and colourleſs as glaſs. Theſe phenomena are more evident, if a phoſphoric ſalt be employed. If two colourleſs tranſparent globules, one formed with borax the other with a phoſphoric ſalt, be fuſed together, a tranſparent compound is produced, which on cooling becomes opake, and of a pearl colour. Meſſrs. H. and B.

With a phoſphoric ſalt.

† Theſe Meſſrs. H. and B. conſider as an acidulous ſulphate of cerium at a maximum of oxidation.

The

The ſulphate of cerium is ſoluble in water only by means of an exceſs of acid. Its taſte is ſaccharine and acid.

With the white oxide. Sulphuric acid eaſily combines with the white oxide, particularly in the ſtate of carbonate. The ſolution is colourleſs, or with a ſlight roſy tinge; of a ſaccharine taſte without any perceptible acidity; and readily affords white cryſtals.

Nitric acid with the red oxide. Nitric acid does not readily diſſolve the red oxide unleſs aſſiſted by heat. If the acid be ſuperabundant, the ſolution yields white deliqueſcent cryſtals: if not, no cryſtals are formed, but a yellowiſh ſalt is formed by deſiccation, of which alcohol at 38° will diſſolve half its weight. The nitrate of cerium is decompoſable by heat, and leaves a brick-coloured oxide.

With the white. The white oxide unites more readily with nitric acid, but this ſalt is not more eaſily cryſtallizable. Its taſte is at firſt pungent, afterward very ſugary.

Muriatic acid. Muriatic acid diſſolves the red oxide with effervescence. The ſolution cryſtallizes confuſedly. The ſalt is deliqueſcent, ſoluble in an equal weight of cold water, and in three or four times its weight in alcohol. The flame of this ſolution acquires no colour from the ſalt, but if agitated, white, red, and purple points appear in it *.

Oxigenated muriatic acid. Oxigenated muriatic acid has no action on the red oxide, but diſſolves the white, without yielding to it any of its oxigen.

Carbonic acid. The oxide of cerium unites eaſily with carbonic acid. The moſt ſimple and ready method of forming this compound is to decompoſe a ſolution of the nitrate or muriate of the white oxide by ſaturated carbonate of potaſh, when a very white precipitate will be formed with effervescence, which is very light, and on drying aſſumes a ſhining ſilvery appearance.

Hidro-ſulphures ſeparate iron from cerium. Sulphurated hidrogen does not combine with cerium: but hidroſulphures may be employed ſucceſſively to ſeparate any iron that may be mixed with it; for, when this is the caſe, the firſt portions of hidroſulphure will throw down from the ſolution of cerium a greeniſh precipitate till no more iron remains.

Tartarous acid. The white oxide will unite directly with tartarous acid, but requires an exceſs of the acid to render it ſoluble †.

Mr.

* When this ſolution is concentrated it burns with a yellow ſparkling flame. Meſſrs. H. and B.

† Meſſrs. H. and B. have obſerved, as well as Mr. V. that, if the

Mr. Vauquelin made ſeveral unſucceſsful attempts to reduce this metal; at firſt he uſed the oxalate made into a paſte with fat oil. However, having mixed tartrite of cerium with a very ſmall quantity of oil and lamp-black, he put it into a crucible of charcoal bedded in ſand in an earthen crucible, and heated it for an hour and half in a forge furnace. A metallic globule ſcarcely as large as a pin's head was now left in the coal, but no other trace of cerium could he diſcovered, though the ſand was examined with the utmoſt care. Reduction of the metal.

None of the ſimple acids acted on this globule, but it diſſolved, though with extreme difficulty, in aqua regia, after being triturated. The ſolution was reddiſh, and exhibited unequivocal marks of iron: but it likewiſe gave evident ſigns of the exiſtence of cerium, both by its ſaccharine taſte, and by the white precipitates which tartrite of potaſh and oxalate of ammonia threw down. The metallic globule too was harder, much more fragile, more ſcaly in its fracture, and more white than pure caſt iron. The globule examined.

As from theſe experiments cerium appears to be volatile, a ſimilar mixture with the addition of borax was heated in a porcelain retort, to the neck of which a porcelain tube was adapted. Whether from the inſufficiency of the heat however, or from the metal being volatilized without adhering to the neck of the retort, no trace of ſublimate was found. But ſeveral very ſmall metallic globules remained in the retort, adhering to a black varniſh formed by the borax. There were ſome of theſe globules about the upper part of the veſſel, to which apparently they had been ſublimed by the force of the fire; but all theſe globules together would not have amounted to a fiftieth part of the cerium employed. Volatile, but attempt to ſublime it fruitleſs.

the ſalts of cerium, decompoſed by tartrite of potaſh ſtill contain traces of iron, the iron remains diſſolved in the liquor, particularly if a ſlight exceſs of tartrite be employed. Accordingly they have propoſed this method as the beſt and ſimpleſt for freeing the cerium from iron. The proceſs they recommend for obtaining pure oxide of cerium is, to diſſolve in nitro-muriatic acid any quantity of cerite, carefully ſelected and thoroughly calcined. To filter the ſolution, neutralize it by cauſtic potaſh, and then precipitate by tartrite and potaſh. The precipitate well waſhed, and afterward calcined, is pure oxide of cerium. Simple and ready method of freeing cerium from iron, and obtaining the oxide pure.

Abſtract

IX.

Abſtract of a Memoir, entitled Conſiderations on Colours, and ſeveral of their ſingular Appearances; read at the Claſs of Mathematical and Phyſical Sciences of the National Inſtitute, March, 1805, *by* C. A. Prieur *.

Object of the memoir.

OUR author here endeavours to account for ſeveral phenomena, which appear to him never yet to have been properly explained: or rather it is his object to exhibit a general theory, by means of which all caſes of coloured appearances, even the moſt extraordinary, may be referred to certain principles.

Begins with the colours reſulting from a mixture of rays.

He ſets out from the known opinions concerning the various ſpecies of luminous rays, the colours reſulting from a mixture of ſeveral of theſe rays taken at different parts of the ſolar ſpectrum, and among others the very remarkable caſe, where the rays are ſo choſen, that their union produces on the organ of ſight the ſenſation of whiteneſs, even if two ſorts of rays only be employed.

For which we are indebted to Newton.

For theſe ideas we are indebted to the diſcoveries of the immortal Newton, and they flow immediately from the method he has propoſed for determining what colour would be obtained from a mixture of certain quantities of other given colours.

Preliminary requiſites.

If we would thoroughly comprehend what paſſes in the ſeeing of colours, it is indiſpenſable in the firſt place to obtain a familiar acquaintance with the ſhades compoſed of ſeveral ſimple rays; to acquire preciſe ideas of *black* and of *white*, and of the complication theſe introduce into coloured appearances; and more eſpecially to underſtand the relation of colours, which, taken two and two in a certain order, are capable of forming by their union white, or if you pleaſe any other compound tint.

Complementary colours.

Two colours having this kind of relation to each other are reciprocally termed *complementary colours*: one of theſe being given, the other may be determined with more or leſs preciſion by various modes of experiment, calculation, or ſimple reaſoning; and the conſideration of them applies very uſefully to a great number of caſes, as will be ſeen farther on.

* Tranſlated from the *Annales de Chimie*, Vol. LIV. p. 5, April, 1805.

We here paſs over many particulars, which perſons verſed in the ſcience of optics, or habituated to the practical application of colours, will eaſily ſupply. Beſides, the ſubſequent part of the memoir, of which we have undertaken to give an account, will furniſh an opportunity of repeating what is moſt neceſſary for underſtanding theſe ſubjects.

Contraſts.

After theſe preliminaries the author proceeds to obſervations on *contraſts*. He employs this word to characterize the effect of the ſimultaneous viſion of two ſubſtances differently coloured, when brought near together under certain circumſtances. Contraſt then is here a compariſon, from which reſults the ſentiment of a certain difference, great or ſmall. It is pretty generally known, and painters in particular are well aware, that a coloured ſubſtance occupying a ſpace of little extent, and placed near or ſurrounded by a given colour, has not the ſame appearance as in the neighbourhood of another colour: but whence ariſes this difference?

Before we attempt to anſwer this queſtion, let us make an eſſential diſtinction. The colours in queſtion muſt be either homogeneal, that is formed of one ſort of rays only; or compound, that is formed of a mixture of different rays.

Contraſts of ſimple colours not yet examined.

In the firſt caſe, it muſt be confeſſed, we are ignorant, whether the approximation of different ſimple colours would produce any alteration in their reſpective appearance. As we ſeldom have an opportunity of ſeeing exhibitions of colour of this kind, and it is not eaſy to arrange ſuch at will, no experiments have yet been made on their contraſts. The ſubject, however, is well worth ſtudying.

Its effect produced by abſtracting from a colour the rays analogous to that contraſted with it.

As to compound colours, and ſuch are almoſt all thoſe of natural or artificial ſubſtances, as our author ſhews in the courſe of his paper, the new colours exhibited by contraſt are always conformable to the tint that would be obtained by abſtracting from the colour proper to one of the ſubſtances the rays analogous to the colour of the other.

Orange on red appears yellow; on yellow, red; on violet, yellowiſh; on green, another red:

Thus if we place on red paper a ſlip painted orange-colour, the latter will appear nearly yellow: on the contrary, the ſame ſtrip placed on yellow paper will appear nearly red. If we place it on violet paper it will reſume a yellowiſh tint, but different from the former; and laſtly, on green paper it will appear red, but in a different degree.

because orange consists of all rays but blue.

The explanation of these instances by the rule proposed is easy, if we suppose the orange-colour of the little strip to be compounded of all the rays except blue, which is commonly the case.

A multitude of combinations of colours thus placed upon one another, bring out the colour of contrast indicated by the rule above laid down; but there are several circumstances that render the effect more striking, or modify the result.

Contrast modified by circumstances.

Degree of light.

Sometimes it depends on the degree of light by which the colours are observed. They may be illumined uniformly, or some more than others. The quantity of light entering simultaneously into the eye from the whole field of view, has likewise its influence. If the colours form several surrounding borders to each other, as a series of circles decreasing in size and placed one upon another would do, they will act reciprocally on each other. At every junction there will be on each side a border coloured by the contrast of the adjacent tint. These borders will be of greater or less extent in proportion to the brightness of the colour. The effect of a single one may be sufficient to deaden or annihilate all the rest.

Many contrasts at once.

Effect increased by slight fatigue of the eye.

The colours of contrast will appear likewise with greater vividness after having observed them a few moments, or if the coloured substances be shaken a little, so that they may pass slowly over the retina. It seems as if a certain fatigue of the eye, either instantaneously with regard to the intensity of the light, or more slowly by a prolonged vision, concurred to produce the appearances in question. But an excessive fatigue of the organ would produce a degeneration of the colours belonging to another mode.

But not by excessive.

Colour on the retina after exposure to strong light, not from contrast.

We ought not therefore to refer to contrast those impressions mentioned by Æpinus, which are propagated in the eye with a certain duration, and a particular period of tints, when we have looked stedfastly on a very brilliant light, as that of the sun.

Buffon's accidental colours are

But the colours termed by Buffon *accidental*, on which Scherfer has written an interesting essay, belong to the class of contrasts, or at least constantly observe the same law.

coloured shadows of the same nature;

Coloured shadows are another phenomenon of the same kind. Count Rumford has established this fact beyond question in

two

wo eſſays, where he has treated the ſubject in a very pleaſing manner.*

Mr. Prieur thinks, that thoſe appearances of the ſolar light received through a hole in a coloured curtain, which General Meuſnier had remarked on account of their ſingularity, are alſo to be aſcribed to contraſt. With this too he aſſimilates ſeveral caſes of colours diſplayed by opals, or, to ſpeak more generally, by bodies including perceptible opake parts diſſeminated through a tranſparent ſubſtance. In the ſame way he explains the colours under which the grayiſh duſt collected by age on papers, or on coloured ſtuffs, appears; and he draws the ſame inferences with reſpect to the blueiſh appearance of the veins of the human body.

alſo light through hole in coloured curtain; opals; old duſt on paper and cloth, and blueneſs of the veins.

He likewiſe propoſes a new method of rendering the colours of contraſt very ſenſible, more ſo than even by the known proceſs of accidental colours, and nevertheleſs without occaſioning any extraordinary fatigue of the eye. This laſt circumſtance is of no ſmall conſequence, for every one muſt be aware, that ſo delicate an organ cannot be ſtrained by over exertion without danger.

This method conſiſts, the obſerver being in a room with a good light, in placing againſt the window the coloured papers, on which he means to obſerve the contraſts in the manner above mentioned. The coloured paper ſerving as the ground will then poſſeſs a degree of ſemitranſparency, while the little ſlip of a different colour placed upon it is more opake, and in the ſhade, on account of the double thickneſs of paper: thus the colour produced by the contraſt is rendered much more ſtriking.

Method of rendering contraſts very ſenſible.

From this arrangement too reſults the ſingularly ſtriking effect of contact of a little ſlip of white paper applied ſucceſſively on paper, glaſs, and cloth of a given colour. When the tranſparent body is red, the opake white appears blueiſh green; if the ground be orange, it is decidedly blue; on a yellow ground, a kind of violet; on a crimſon ground, green, &c.; always correſponding exactly to the *complementary colour*.

Slip of white paper on coloured paper, glaſs, &c.

On this it muſt be obſerved, that, according to the rule already mentioned, if we abſtract from white, which is a com-

Explanation.

* See his Philoſophical Works, Vol. I, p. 319, and following.

pound of all the coloured rays, the red rays for example, the remaining pencil ought to appear a very pale blueiſh green: but, as in the experiment above the little white ſlip is in the ſhade, the black hence ariſing may be of a proper degree to deſtroy the effect of the white, and then the blueiſh green appears of a lively tint. The ſame reaſoning is applicable to the caſe of all the other colours.

Reflected light muſt be avoided.

To obtain the full effect in repeating theſe experiments, we muſt take care, while procuring a favourable light, to guard againſt the reflection of adjacent bodies, and againſt double coloured fringes. Thus when the bright light tranſmitted through the window ſurrounds the tranſparent paper, it may very ſenſibly augment the brightneſs of the colour of contraſt, or injure it by introducing another tint, according to the colour of the body under obſervation. We have it always in our power, however, to get rid of this ſupercompoſition, by taking a piece of black cloth or paſteboard to maſk the object thus incommoded, or by looking through a blackened tube ſo as to confine the field of viſion to the neceſſary extent.

How.

Uſeful in the arts.

This knowledge of contraſt may be uſefully applied to thoſe arts, which are employed on the ſubject of colours. The painter is aware, that it is not a matter of indifference what colour is placed near another: but when he is acquainted with the law, to which their action on each other is ſubjected, he will know better what to avoid, and how to diſpoſe his tints, ſo as to heighten the brilliancy of that which he wiſhes to bring forward. Contraſting them together in ſucceſſion likewiſe affords us valuable indications of their nature and compoſition. This the author himſelf has put in practice with advantage in his manufactory of colours and paper-hangings.

White appearance of a coloured body through glaſs of the ſame hue.

Theſe conſiderations on contraſts led him to the examination of a very ſingular caſe, which Mr. Monge has mentioned and treated with his uſual ſagacity *. This caſe is the white appearance, which a coloured body ſometimes exhibits when viewed through a glaſs of the ſame hue. There remained ſome uncertainty reſpecting the circumſtances actually neceſſary for producing this effect: theſe our author determines by particular experiments, and he enumerates thoſe which have a favourable influence or the contrary. His concluſion is, that,

* *Annales de Chimie*, Vol. III.

when

when we have the perception of whiteneſs in theſe caſes, it is owing ſolely to the action of contraſts, by which the impreſſion of the colour is deadened or annihilated; while that of a certain degree of brightneſs ſtill ſubſiſts, and is noticed from the oppoſition of a greater degree of obſcurity. This manner of conſidering the ſubject leads to a new definition of whiteneſs which has certainly nothing in it inconſiſtent: *white is with reſpect to us the ſenſation of light, when no particular colour predominates in it, or is perceived in it.*

New definition of whiteneſs.

In the ſubſequent part of his memoir our author particularly conſiders the colouring of different opake and tranſparent bodies; that is to ſay, he inquires what are the luminous rays which a given coloured body is really capable of reflecting or tranſmitting.

Further ſubject of inquiry.

His method of making his experiments is ſimple. If the ſubſtance be opake, he places it on a piece of black cloth, and obſerves it with the priſm. If it cannot be cut ſo as to reduce it to a rectangular figure, he covers it with a piece of blackened paſteboard, in which there is a rectangular aperture. Under theſe circumſtances the coloured fringes diſplayed on two oppoſite ſides indicate the kind of rays reflected, and conſequently thoſe abſorbed when we know the nature of the illuminating pencil. On which we have farther to remark, that, as the fringes are themſelves compound tints, the ſimple tints that compoſe them muſt be diſcriminated. Their inſpection ſuffices an experienced perſon for this; but the habit is to be acquired, and its place ſupplied, by taking for a guide papers repreſenting each ſpecies of rays, placing them in their order one upon another, and drawing them back in gradation conformably to their difference of refrangibility: or we may uſe a table conſtructed after Newton's method for determining the compound tints of ſeveral elementary colours.

Method of making his experiments.

Compound tints to be diſcriminated.

How.

If the body to be examined be tranſparent, the aperture in the paſteboard juſt mentioned will be well adapted to cover it when placed againſt the light, ſo that the priſm may exhibit fringes on it. Or, if the obſerver place himſelf in the dark, a light, as that of a candle, will exhibit through the tranſparent ſubſtance, by the aſſiſtance of the priſm, a ſeries of coloured images correſponding to the rays tranſmitted.

Method of examining tranſparent bodies.

Making his experiments in this manner, our author diſcovered that ſeveral opake ſubſtances which happened to be at hand,

Colours of opake bodies owing to abſorption.

hand, of various natures and of all colours, whether yellow, orange, red, or green, blue and violet, owed their coloured appearance to the following laws: 1ſt, each of the bodies always abſorbed the rays that were complementary to the predominant colour: 2dly, in ſome the abſorption included, beſide the complementary ſpecies, others collateral to this ſpecies, and more or leſs numerous: 3dly, the deeper a colour is, the fewer ſpecies of rays it reflects.

Laws.

Relates to chemical compounds, not mechanical mixtures.

It is to be underſtood that mixed colours are not here ſpoken of, but only thoſe that form a homogeneal compound, or a true *combination*, in the ſenſe in which chemiſts uſe this word. Nor muſt the colour reflected from the interior of the molecules, ſuſceptible of light or deep tints, be confounded with the light reflected from the anterior ſurface of bodies: and though this mixes more or leſs with the proper colour, it is eaſy to diminiſh its effects, and diſcriminate them in the experiments.

Predominant colour.

Another remark proper to be made, is, that the expreſſion *predominant colour* muſt not be ſuppoſed to imply, that the rays of this colour are more abundant than the reſt, which would be a miſtake. Several ſpecies of rays may exiſt together in the pencil producing the colour, without any one ſpecies being for this reaſon more abundant. Strictly ſpeaking, all the elements of the pencil are diſſimilar; and conſequently no one exiſts in it in greater quantity. But the general tone of colour remains analogous to that of the rays ſtyled predominant; for which reaſon it is well to retain the term, provided it be not taken in an exaggerated ſenſe.

Tranſparent bodies follow the ſame law of abſorption.

The author has likewiſe obſerved tranſparent bodies, ſuch as coloured glaſs of different ſorts, and liquors contained in a bottle with two broad parallel ſides. For theſe he found the ſame law of abſorption as for opake bodies, but ſtill more marked, and free from all doubt.

Its modifications.

This law is conſtant and regular. It depends on the nature of the body receiving the light, its denſity, and its thickneſs. It is likewiſe modified by the intenſity of the light of the illuminating body, and the kind of rays that compoſe this light.

Progreſs of the abſorption of rays.

The abſorption always commences with the rays moſt oppoſite to the predominant colour of the body illumined. It goes on to thoſe which come next in the ſpectrum; and thus proceeds regularly from one order of rays to the next in ſucceſſion,

ceſſion, never by fits, till it reaches the laſt. In conſequence the body grows darker and darker, and always finiſhes with becoming black. Sometimes it extends only on one ſide from the rays firſt abſorbed; at other times on both ſides at once, and either with equal pace, or more rapidly on one ſide than on the other.

Change of circumſtances varies the reſults.

If we vary each particular that affects the experiment ſeparately, we ſhall have a diſtinct progreſſion of reſults. That depending on the denſity of the ſubſtance is not always ſimilar to that ariſing from change of thickneſs. In receiving light of different kinds too on the ſame ſubſtance, the progreſs of abſorption is differently modified, and conſequently the colours changed.

Our author adduces inſtances of all theſe caſes. He takes them from the numerous experiments he has made with coloured glaſs, acid, or alcaline ſolutions of metals, and fluids tinged by the infuſion or decoction of vegetable ſubſtances. Theſe exhibit curious particularities, but we ſhall not here relate them, both for the ſake of brevity, and becauſe it is eaſy for any perſon to obſerve them, when once the track is pointed out.

May lead to the cauſe of permanent colours.

From all theſe obſervations taken together, many very important conſequences reſpecting the reciprocal action of bodies and light on each other are drawn; and perhaps at ſome future period they will tend to elucidate the grand queſtion concerning the cauſe to which their permanent colours are to be aſcribed.

Colour of bodies at different degrees of incandeſcence.

After theſe hints, the author dedicates a concluding paragraph to the examination of ſeveral phenomena of different kinds. He points out the modifications that coals heated to different degrees of incandeſcence undergo in their colours.

Lamps ſeen through miſt. Sun through ſmoaked glaſs.

His remarks apply to other ſubſtances likewiſe, as iron in the ſtate of ignition, a long row of lamps with reverberators ſeen through a fog, or a white light ſeen through a glaſs blackened by progreſſive applications of ſmoke. In all theſe caſes the colours neceſſarily paſs through a ſeries of tints from white to yellow, orange, and red of a deeper and deeper ſhade, the reaſon of which he gives.

Colours of metallic oxides proportionate to the oxigen.

Metallic oxides too have a gradation of tints, according to their proportion of oxigen. A certain continued change in vegetation

Of flowers, &c. vegetation produces the ſame effect on ſome parts of plants. The arts and chemical proceſſes exhibit the ſame in a multitude of circumſtances.

Uſe to the manufacturer. Hence the manufacturer may derive with advantage indications either of the progreſs of combinations, or of the proper inſtant for executing certain parts of his operations.

Coloured clouds. Our author next enters more particularly into the appearance of coloured clouds, particularly thoſe we ſee about the riſing and ſetting of the ſun. This phenomenon ſo generally known, had hitherto remained without explanation, though this had been attempted by natural philoſophers of the firſt rank.

Owing to abſorption of light, not to refraction It is not owing to the refraction of the ſolar rays, but to the ſucceſſive abſorption of them, when they ſtrike on the inferior parts of the atmoſphere, which are more loaded with vapour.

This abſorption follows laws analogous to thoſe already mentioned. The quantity of vapours, and even their nature not being the ſame every day, produce correſponding differences in their effects.

Order of abſorption. Commonly the firſt rays attacked by theſe vapours are the blue adjacent to the violet. Soon after they attack the contiguous rays, gaining with more rapidity the blue properly ſo called; then the green, the yellow, and thus proceeding to the red. Hence the yellowiſh, orange, and red colours

Sun-ſet. exhibited by the clouds. This period of tints, the evening for example, diſplays itſelf gradually as the ſun approaches the horizon. The ſame hues tinge terreſtrial objects, the part of the air neareſt the ſun, and this luminary itſelf. Accordingly when we can receive its rays on a priſm, we perceive that the rays actually abſorbed correſpond to the general tint of the moment.

From the ſucceſſive increaſe of the vapours traverſed by the light in thickneſs and denſity, it follows likewiſe, that at the ſame inſtant clouds differently placed muſt be clothed in different hues. The higheſt may be white, while others not ſo high are yellow, and others ſtill lower proportionately more red. At equal elevations thoſe furtheſt from the point where the ſun ſets will incline to red, and thoſe neareſt it to yellow.

Blue and green ſhadows, owing to contraſt. We may then ſee blue or green ſhadows on bodies naturally white, as Buffon and other philoſophers have obſerved. Theſe, as has been ſaid, are nothing more than the effect of contraſt between

between the actual colour of the part enlightened, and that of the part in ſhade.

Contraſts may likewiſe render the colour of the clouds complicated, as for inſtance, when a great portion of the ſky diſplays its blue tint. There are ſome clouds, the colour of which ariſes ſolely from this cauſe; and ſuch may be ſeen at times in the middle of the day, when we have a lofty mountain at our back, or are in any other ſituation where the eye is defended from the too powerful action of the ſolar light, either direct or reflected; but in this caſe the clouds have only a yellowiſh tinge, preciſely the complementary colour of ſky blue. Contraſts affect the colour of the clouds.

Sometimes we ſee the moon of a ſimilar colour, when it is very high, a little before or after the ſun paſſes the the horizon: farther it appears thus, or even completely white, when clouds variouſly coloured by the vapors of ſunſet or ſun-riſe exiſt in the air at the ſame time. From this concurrence of circumſtances we have a new proof of the difference of cauſes to which theſe colours are owing. The Moon.

Laſtly let us remark, that from the irregularity of the earth's ſurface, and of the ſtate of the atmoſphere, the phenomena are liable to be concealed or ſubjected to various interruptions. In our climate the colouring of the clouds ſeldom reaches its laſt ſtage. On ſome evenings however, when the ſky is very clear toward the part where the Sun ſets, while light clouds float very high over our heads, we ſhall ſee theſe at a ſubſequent period appearing of a very light red, heightened by the diminution of light on the earth, ſoon after obſcured, and at length becoming extinct in ſhade. Red clouds over head at ſun-ſet.

Concluſion.

Notwithſtanding the many beautiful diſcoveries already made reſpecting light, the theory of the production of colours has not yet attained a degree of generalization that renders it applicable to all caſes, or that ſimplicity of principles to which we are almoſt always led when we have diſcovered the real laws of nature. Many phenomena have eluded explanation, and that given of ſeveral requires correction. Our author has propoſed to eſtabliſh alterations in the theory, the neceſſity of which he points out. He ſupports his principles partly by the doctrine and facts generally admitted; partly by The theory of colours imperfect.

by others leſs commonly known, though of ancient date; and laſtly by obſervations of his own. He is far from flattering himſelf, however, that a ſketch like the preſent exhibits the matter in a ſuitable light; and was ſoon aware that a ſubject ſo extenſive and ſo complicated required maturer labours.

The author intends to purſue the ſubject.

To fill up many gaps, unfold various points, and correct and extend others by farther reſearches, new experiments, and profound reflections, is an ample field of improvement; and this he will attempt, if his powers and his leiſure will permit.

It would likewiſe be uſeful, as well as juſt, to give at the ſame time an abſtract of what we owe to the genius of the great Newton, who opened the career in ſuch an admirable manner, and to thoſe philoſophers who have diſcovered new facts, or removed difficulties. Greater preciſion alſo ſhould be introduced into the language which we employ reſpecting colours, proportionate to the increaſe of our knowledge, and the actual ſtate of the arts and ſciences. Laſtly, in a ſubject like the preſent, it would not be too much to add the reſources of algebra and geometry to the treaſures of experiment, and if poſſible to the advantages of a better method.

X.

Report made by the Phyſical and Mathematical Claſs of the Inſtitute in Anſwer to the Queſtion, whether thoſe Manufactories, from which a diſagreeable Smell ariſes, may prove injurious to Health. Read in the Sitting of January, 1805, *by Meſſrs.* GUYTON-MORVEAU and CHAPTAL.*

THE miniſter of the home-department has conſulted the claſs on a queſtion, the ſolution of which is of eſſential import to our manufacturers.

Queſtion.

The object is to determine, whether the vicinity of certain manufactories can be injurious to health.

Its importance.

The ſolution of this problem muſt appear of the more conſequence, as, from the confidence which the deciſions of the Inſtitute naturally merit, it may hereafter form the baſis of

* Tranſlated from the *Annales de Chimie*, vol. LIV. p. 86, for April, 1805.

deciſions

deciſions in a court of juſtice, when ſentence is to be pronounced between the fate of a manufactory and the health of our fellow-citizens.

The ſolution is ſo much the more important, it is become ſo much the more neceſſary, as the fate of the moſt uſeful eſtabliſhments, I will ſay more, the exiſtence of many arts, has depended hitherto on ſimple regulations of police; and that ſome, driven to a diſtance from materials, from workmen, or from conſumers, by prejudice, ignorance, or jealouſy, continue to maintain a diſadvantageous ſtruggle againſt innumerable obſtacles, by which their growth is oppoſed.

Manufactories objected to.

Thus we have ſeen manufactories of acids, of ſal ammoniac, of Pruſſian blue, of beer, and of leather, ſucceſſively baniſhed from cities; and we daily ſee appeals to authority againſt theſe eſtabliſhments made by troubleſome neighbours or jealous rivals.

Diſadvantage of having no fixed rules.

As long as the fate of theſe manufactories is inſecure, as long as an arbitrary legiſlation poſſeſſes a right to interrupt, ſuſpend, or fetter the hands of a manufacture; in a word, as long as a ſimple magiſtrate of police has at his nod the fortune or ruin of a manufacturer, how can we conceive, that he will be ſo imprudent as to engage in undertakings of ſuch a nature? How could it be expected, that manufacturing induſtry ſhould eſtabliſh itſelf on ſuch a frail baſis? This ſtate of uncertainty, this continual conteſt between the manufacturer and his neighbours, this perpetual doubt reſpecting the fate of an eſtabliſhment, paralyſe and confine the efforts of the manufacturer, and gradually extinguiſh both his courage and his powers.

It is an object of primary neceſſity therefore to the proſperity of the arts, that lines ſhould be drawn, ſo as no longer to leave any thing at the arbitrary will of the magiſtrate; to point out to the manufacturer the circle in which he may exert his induſtry with freedom and ſecurity, and to aſſure the neighbouring proprietor, that he has nothing to fear for his health, or for the produce of his fields.

To arrive at the ſolution of this important problem, it appeared to us indiſpenſable, that we ſhould take a view of each of the arts, againſt which the moſt clamour has been raiſed.

Claſſification of objectionable trades.

With this view we ſhall divide them into two claſſes. The firſt will compriſe all thoſe, the proceſſes of which allow aeriform emanations to eſcape from them into the atmoſphere, either

in

in conſequence of putrefaction or fermentation, which may be deemed nuiſances from their ſmell, or dangerous from their effects.

The ſecond claſs will include all thoſe, in which the artiſt, operating by the aid of fire, developes and evolves in air or vapour various principles, which are more or leſs diſagreeable to reſpire, and reputed more or leſs injurious to health.

1ſt claſs. In the firſt claſs we may advert to the ſteeping of flax and hemp, the making of catgut, ſlaughter-houſes, ſtarch-manufactories, tanneries, breweries, &c.

2d claſs. In the ſecond, the diſtillation of acids, of ſpirits, and of animal ſubſtances; gilding on metals, preparations of lead, copper, and mercury, &c.

1ſt claſs injurious to health. The arts of the firſt claſs, conſidered in relation to the health of the public, merit particular attention, becauſe the emanations that proceed from fermentation or putrefaction are really injurious to health in ſome caſes, and under certain circumſtances: the ſteeping of flax and hemp for inſtance, which is performed in ponds or ſtill waters, infects the air and kills fiſhes; and the diſeaſes to which it gives riſe are all known and deſcribed: Accordingly wiſe regulations have almoſt every where enjoined, that this operation ſhould be carried on without the precincts of towns, at a certain diſtance from every dwelling, and in waters, the fiſh of which conſtitute no reſource for the public. Theſe regulations unqueſtionably ought to be continued; but as the execution of them is attended with ſome inconvenience, it is to be wiſhed, that the proceſs of Mr. Brale, the ſuperiority of which has been confirmed by Meſſrs. Mongez, Berthollet, Teſſier, and Molard, ſhould ſoon become known and adopted.

Steeping hemp.

Brale's method recommended.

Beer, vegetable colours, ſtarch, paper, &c. Other operations on vegetables, or certain products of vegetation, to obtain fermented liquors, as in breweries; to extract colours, as in the manufactures of litmus, archil, and indigo; or to diveſt them of ſome of their principles, as in manufactories of ſtarch, paper, &c. do not appear to us of ſuch a nature as to be capable of exciting any diſquietude in the mind of the magiſtrate. At all events the emanations ariſing from theſe ſubſtances in a ſtate of fermentation can prove dangerous only near the veſſels and apparatus in which they are confined, ceaſing to be ſo the moment they are mingled with the open air; ſo that a little prudence only is required, to avoid all

danger

danger from them. Beſides, the danger affects only the manufacturers themſelves, and by no means the inhabitants of the neighbouring houſes, ſo that a regulation enjoining theſe manufactories to be removed out of towns, and to a diſtance from any dwelling-houſe, would be an act of authority both unjuſt, vexatious, and injurious to the progreſs of manufactures, and in no reſpect a remedy for the evils attending the operation.

Some preparations extracted from animal ſubſtances require the putrefaction of theſe ſubſtances, as in the fabrication of catgut; but it is more frequently the caſe, that animal ſubſtances employed in manufactures are liable to putrefaction from being kept too long, or expoſed to too great warmth, as we particularly find in dyeing cotton red, a proceſs in which a large quantity of blood is employed. The miaſmata exhaled by theſe putrid matters ſpread far round, and form a very diſagreeable atmoſphere for all the neighbourhood to breathe; it is the part of a good government, therefore, to cauſe theſe ſubſtances to be renewed ſo as to prevent putrefaction, and the manufactory to be kept ſo far clean, that no refuſe of the animal ſubſtances employed ſhall be left to rot in them.

Catgut.

Dyeing cotton red.

In this laſt point of view ſlaughter-houſes exhibit ſome inconveniencies; but they are not of ſufficient importance to require them to be placed without the precincts of towns, and aſſembled together in one ſpot, as ſpeculative men are daily propoſing to government. A little attention on the part of the magiſtrate, to prevent butchers from throwing out the blood and refuſe of the beaſts they kill, would be ſufficient to remedy completely every thing diſguſting or unhealthy ariſing from ſlaughter-houſes.

Slaughter-houſes.

The fabrication of *poudrette* (night-ſoil dried) begins to be eſtabliſhed in all the large towns of France, and the operation by which excrementitious ſubſtances are reduced to this ſtate, neceſſarily occaſions a very diſagreeable ſmell for a long time. Eſtabliſhments of this kind therefore ought to be confined to airy places, remote from any habitation; not that we conſider the aeriform exhalations from them as injurious to health; but no one can deny, that they are incommoding, noiſome, diſagreeable, and difficult to breathe, on all which accounts they ought to be removed to a diſtance from the dwellings of men.

Poudrette.

There is a very important obſervation to be made on the ſpontaneous decompoſition of animal ſubſtances, which is, that

Animal putrefaction dangerous only in proportion to its humidity.

that the emanations from them appear to be ſo much the leſs dangerous, as the ſubſtances which undergo putrefaction are leſs humid: in the latter caſe, a conſiderable quantity of carbonate of ammonia is evolved, which imparts its predominant character to the other matters volatiliſed, and corrects the bad effects of ſuch as are deleterious. Thus the decompoſition of ſtercoraceous matters in the open air, and in places the ſituation and declivity of which allow the fluids to drain off, and that of the refuſe of the cocoons of the ſilk-worm evolve a vaſt quantity of carbonate of ammonia, which corrects the virus of ſome other emanations; while the very ſame ſubſtances, decompoſed in water or drenched with this fluid, exhale ſweetiſh and nauſeous miaſmata, the reſpiration of which is very dangerous.

2d claſs. The numerous arts in which the manufacturer produces and diffuſes in the air, in conſequence of his proceſſes and by the help of fire, vapours more or leſs diſagreeable to breathe, conſtitute the ſecond claſs of thoſe we have to examine.

Theſe, more intereſting than the former, and much more intimately connected with the proſperity of our national induſtry, are ſtill oftener the ſubject of complaints brought before the magiſtrate for deciſion, and on this account have appeared to us to require more particular attention.

We will begin our examination with the manufacture of acids.

Acids. The acids that may excite complaints of the neighbours againſt their preparation are the ſulphuric, nitric, muriatic and acetous.

Sulphuric acid. The ſulphuric acid is obtained by the combuſtion of a mixture of ſulphur and nitre. It is very difficult in this proceſs to prevent a more or leſs obſervable ſmell of ſulphurous acid from being diffuſed around the apparatus, in which the combuſtion is performed; but in manufactories ſkilfully conducted this ſmell is ſcarcely perceptible within the building itſelf, is not dangerous to the workmen who reſpire it daily, and can give no reaſonable foundation for complaint to the neighbours. When the art of making ſulphuric acid was introduced into France, the public opinion was ſtrongly expreſſed againſt the firſt eſtabliſhments for the purpoſe; the ſmell of the match with which we kindle our fires contributed not a little to exaggerate the effect that muſt be produced by the rapid com-

buſtion of ſeveral hundred weight of brimſtone; but men's fears on this head are now ſo much allayed, that we ſee ſeveral of theſe manufactories proſper in peace in the midſt of our cities.

Aqua fortis and ſpirit of ſalt.

The diſtillation of aqua fortis and ſpirit of ſalt, in other words, of the nitric and muriatic acids, are not more dangerous than that of ſulphuric acid. The whole of the proceſs is performed in an apparatus of glaſs or earthen-ware, and it is unqueſtionably the great intereſt of the manufacturer to diminiſh the volatilization or loſs of the acid as much as poſſible. Yet, let him pay whatever attention he will to this, the air breathed in the manufactory is always impregnated with the ſmell peculiar to each of theſe acids; but you may reſpire there freely and ſafely, the men who work in it daily are not at all incommoded by it, and the neighbours would be very much in the wrong to complain.

Vinegar.

Since the manufactories of white lead, of verdigris, and of ſugar of lead have increaſed in France, the demand for vinegar has been enlarged.

Diſtilled vinegar.

When this acid is diſtilled, to fit it for ſome of the purpoſes for which it is uſed, it diffuſes to a diſtance a very ſtrong ſmell of vinegar, in which there is no danger; but when a ſolution of lead in this acid is evaporated, the vapours aſſume a ſweetiſh character, and produce in thoſe who reſpire them conſtantly all the effects peculiar to the emanations of lead itſelf. Happily theſe effects are confined to the people who work in the manufactory, and are unfelt by thoſe who dwell in the vicinity.

Mineral preparations, and gelding.

The preparations of mercury and of lead, thoſe of copper, antimony, and arſenic, and the proceſſes of gilding on metals, are none of them without ſome danger to the perſons who reſide in thoſe manufactories, and are concerned in the operations; but their effects are bounded by the walls within which they are carried on, and are dangerous only to the perſons concerned in the manufactories. It is an object well worthy the attention of chemiſts, to inveſtigate the means of preventing theſe injurious effects, and indeed many of the inconveniencies have already been prevented by the help of chimneys, which convey the vapours into the air out of the reach of reſpiration; and at preſent the whole attention of adminiſtration

ought

ought to be confined to directing ſcience toward the means of improvement of which theſe proceſſes are ſuſceptible with regard to health.

Pruſſian blue, and ſal ammoniac.

The fabrication of Pruſſian blue, and the extraction of carbonate of ammonia by the diſtillation of animal ſubſtances in the new manufactories of ſal ammoniac, produce a large quantity of fetid vapours or exhalations. Theſe exhalations, it is true, are not injurious to health; but as it is not ſufficient to conſtitute a good neighbour, not to be a dangerous one merely, but not even to be a diſagreeable one, they who undertake ſuch manufactures, when they have to ſeek a ſituation for them, ſhould prefer one remote from any dwelling-houſe. But when ſuch a manufactory is already eſtabliſhed, we would be far from adviſing the magiſtrate to order its removal: it would be ſufficient in ſuch caſes, to oblige the manufacturer to build very high chimneys, that the diſagreeable vapours produced in theſe operations may be diſſipated in the air. This is particularly practicable for the fabrication of Pruſſian blue, and by adopting it one of our number has continued to retain in the midſt of Paris one of the moſt important manufactories of this kind we have, againſt which the neighbours had already leagued.

Few injurious to health.

In the report we lay before the claſs we have thought it our duty to attend only to the principal manufactories, againſt which violent clamours have been raiſed at divers times and places. It is eaſy to ſee, from what has been ſaid, that there are but few the vicinity of which is injurious to health.

Caution to magiſtrates.

Hence we cannot too ſtrongly exhort thoſe magiſtrates who have the health and ſafety of the public committed to their charge, to diſregard the unfounded complaints, which, too frequently brought againſt different eſtabliſhments, daily threaten the proſperity of the honeſt manufacturer, check the progreſs of induſtry, and endanger the fate of art itſelf.

They ſhould not liſten too readily to complaints.

The magiſtrate ought to be on his guard againſt the proceedings of a reſtleſs or jealous neighbour; he ſhould carefully diſtinguiſh what is only diſagreeable or inconvenient from what is dangerous or injurious; he ſhould recollect that the uſe of pit-coal was long preſcribed, under the frivolous pretence that it was injurious to health; in ſhort, he ſhould be fully aware of this truth, that, by liſtening to complaints of this nature, not only would the eſtabliſhment of ſeveral uſeful arts in France be

be prevented, but we ſhould inſenſibly drive out of our cities the farriers, carpenters, joiners, braſiers, coopers, founders, weavers, and all whoſe occupation is more or leſs diſagreeable to their neighbours. For certainly the employments juſt named are more unpleaſant to live near than the manufactories mentioned above, and the only advantage they enjoy is that of ancient practice. This right of toleration has been eſtabliſhed by time and neceſſity; let us not doubt therefore, but our manufactures, when grown older and better known, will peaceably enjoy the ſame advantage in ſociety; in the mean time we are of opinion, that the claſs ought to avail itſelf of this circumſtance, to put them in a particular manner under the protection of government, and declare publicly, that the manufactures of acids, ſal ammoniac, Pruſſian blue, ſugar of lead, white lead, ſtarch, beer, and leather, as well as ſlaughter-houſes, are not injurious to the health of the vicinity, when they are properly conducted.

Diſagreeable occupations ſanctioned by time.

Manufactures not injurious to health.

We cannot ſay as much for the ſteeping of hemp, making catgut, layſtalls, and in general eſtabliſhments where a large quantity of animal or vegetable matter is ſubjected to humid putrefaction. In all theſe caſes, beſide the diſagreeable ſmell they exhale, miaſmata, more or leſs deleterious, are evolved.

Injurious manufactures.

We muſt add, that, though the manufactories of which we have already ſpoken, and which we have conſidered as not injurious to the health of the neighbourhood, ought not to be removed, yet adminiſtration ſhould be requeſted to watch over them ſtrictly, and conſult with well-informed perſons for preſcribing to the conductors the moſt proper meaſures for preventing their ſmoke and ſmell from being diffuſed in the vicinity. This end may be attained by improving the proceſſes of the manufactures, raiſing the outer walls, ſo that the vapours may not be diffuſed among the neighbours; improving the management of the fires, which may be done to ſuch a point, that all the ſmoke ſhall be burnt in the fire-place, or depoſited in the tunnels of long chimneys; and maintaining the utmoſt cleanlineſs in the manufactories, ſo that nothing ſhall be left to putrify in them, and all the refuſe capable of fermentation be loſt in deep wells, and prevented from any way incommoding the neighbours.

Manufactures not injurious require ſome reſtrictions.

We ſhall obſerve too, that when new manufactories of Pruſſian blue, ſal ammoniac, leather, ſtarch, or any other ar-

New manufactories.

ticle by which vapours very inconvenient to the neighbours, or danger of fire or explosions are to be established, it would be wise, just, and prudent, to lay down as a principle, that they are not to be admitted into cities, or near dwellings, without special authority; and that, if persons neglect to comply with this indispensable condition, their manufactories may be ordered to be removed without any indemnification.

Summary. It follows from our report, 1st, that catgut manufactories, laystalls, steeping of hemp, and every establishment in which animal or vegetable matters are heaped together to putrify in large quantities, are injurious to health, and ought to be remote from towns and every dwelling house: 2dly, that manufactories where disagreeable smells are occasioned through the action of fire, as in the making of acids, Prussian blue, and sal ammoniac, are dangerous to the neighbours only from want of due precautions, and that the care of government should extend only to an active and enlightened superintendance, having for its objects the improvement of their processes and of the management of the fire, and the maintenance of cleanliness: 3dly, that it would be worthy a good and wise government, to make regulations prohibiting the future establishment of any manufacture, the vicinity of which is attended with any essential inconvenience or danger, in towns or near dwelling-houses, without special authority previously obtained. In this class may be comprised the manufactories of poudrette, leather, and starch; foundries, melting houses for tallow, slaughter houses, rag warehouses, manufactories of Prussian blue, varnish, glue and sal ammoniac, potteries, &c.

Such are the conclusions which we have the honour to lay before the class,* and addressed to government, with invitation to make it the base of its decisions.

* These conclusions were adopted by the Institute.

Facts

XI.

Facts relative to the Torpid State of the North American Alligator.
By BENJAMIN SMITH BARTON, M. D. *.

IT has not, I think, been remarked by the generality of the writers on natural hiſtory, that the North American Alligator paſſes during the prevalence of cold weather, into the torpid ſtate. This however, is unqueſtionably the caſe in ſome parts of the continent.

Account by by Boſſu.

Mr. Boſſu, a French writer, after telling us that theſe animals are numerons in the Red River, one of the weſtern branches of the Miſſiſſippi, ſays, " they are torpid during the cold weather, and lie in the mud with their mouths open, into which the fiſh enter as into a funnel, and neither advance nor go back. The Indians then get upon their backs, and kill them by ſtriking their heads with hatchets, and this is a kind of diverſion for them †.

Dr. Foſter, the tranſlator of the work, obſerves in the preceding paſſage, " that the circumſtance of the alligator's being torpid during winter is quite new, and very remarkable for natural hiſtory." It ſeems (he adds) almoſt all the claſs of animals called *amphibia*, by Dr. Linnæus, when found in cold climates grow torpid during winter.

Another account by Mr. Graham.

In addition to the authority of Mr. Boſſu, I may here mention the following fact, which was communicated to me about the year 1785, by a Mr. Graham, at that time a very intelligent ſtudent of medicine in the Univerſity of Pennſylvania.

The alligator ſwallows pine-knots previous to becoming torpid.

" The alligator having previouſly ſwallowed a number of pine-knots, retires to his hole, where he remains in a torpid ſtate, during the ſeverity of winter. If killed at this ſeaſon, theſe knots are found highly poliſhed by their trituration one againſt the other in the animal's ſtomach, as I have more than once heard from men of undoubted veracity, who had

* From " the Philadelphia Medical and Phyſical Journal". Collected and arranged by Ben. Smith Barton, M. D. It is publiſhed in half yearly Numbers, the firſt of which appeared in November, 1804.

† Travels through that part of North America formerly called Louiſiana. Engliſh Tranſlation, Vol. I. p. 367. London 1771.

been concerned in the fact. Indeed this is so notorious in those parts in which these creatures abound, that the digestion of the alligator's stomach is proverbial amongst the multitude, who deride its insipidity in the choice of such food, though, I presume, this it does instinctively, for some purpose unaccounted for by naturalists; and which, perhaps is beyond the limits of human ken."

The fact related by Mr. Graham, relates to the alligator of the Carolinas, in which parts of the United States this animal is very common. By another gentleman I have been informed, that the pine knots which the alligators swallow are generally such as are very abundant in turpentine. I have also been assured, by my friend Mr. William Bartram, that he has seen a brick-bat which was taken out of the stomach of an alligator, and that it was worn quite round.

Local situation of this animal, &c.

Mr. Lawson says, that the alligator is not seen to the North of North-Carolina. They are very common at Cape-Fear in latitude 34. One twelve feet in length has been seen at this place. On the Atlantic side of the United States I am not able to trace them farther than the "Alligator Dismal Swamp," which is between Edenton and Newbern in North-Carolina. The mouth of the Red River in latitude 31.

Within the tract of country just mentioned, the alligator obeying the impulse of the climate, passes into the torpid state. In North-Carolina this takes place about the middle of November, sooner or later, according to the state of the season. Whether the animal becomes torpid in more Southern parts of the Continent, I have not been able to learn. On the river St. John in East Florida, they have been seen awake even in the middle of winter, but it was remarked that they seemed dull and stupid. It has also been observed, that they are accustomed to frequent the warm springs which are so abundant in this part of the Continent; and that they are fond of lying in these springs. Perhaps the heat of these springs may be sufficient to prevent them from becoming torpid. But it must be observed, that a deficiency of heat is not the only cause of the torpid condition of animals.

Conjectures respecting their swallowing the knots of the pine.

It may not perhaps be an easy task to assign a satisfactory cause for the singular instinctive appetite, which leads the alligator, before going into the torpid state, to swallow pine-knots, and other somewhat similar substances. But I ap-

prehend that theſe ſubſtances, when taken in by the animal, act in ſome meaſure by keeping up a certain degree of action in its ſtomach, and conſequently in every part of the ſyſtem, and thereby prevents the death of the animal, which might otherwiſe be deſtroyed by the long continued application of cold. Some facts mentioned by Dr. Pallas, though they reſpect a very different family of animals, render this conjecture not a little plauſible*.

This ſubject is worthy of more attention. In particular, it will be well to enquire, whether the alligator does ſwallow pine-knots, ſtones, &c. in thoſe parts of America in which it does not paſs into the torpid ſtate.

XII.

Obſervations and Experiments on the conducting Power of Fluids. By T. S. Traill, *M. D. From the Author.*

To Mr. NICHOLSON.

SIR, *Liverpool, Sept.* 10, 1805.

If you think the following obſervations and experiments worthy of a place in your excellent Journal, your inſerting them will oblige,

Your obedient ſervant,

J. S. TRAILL, *M. D.*

Doctrine of Count Rumford, that fluids are non-conductors of heat, controverted.

Count Rumford was the firſt who maintained that fluids are abſolute non-conductors of caloric. This concluſion he drew from the intereſting fact he had diſcovered, of the extreme ſlowneſs with which ice melted when a ſtratum of cold water was interpoſed between it and the heated body. He imagines that it was always melted in ſuch circumſtances, either by *currents* produced, in ſome of them by changes in ſpecific gravity, or by the tranſmiſſion of caloric through the ſides of the containing veſſel. The experiments of this illuſtrious philoſopher have rouſed the attention of the learned, and to the united labours of yourſelf, of Thomſon, of Dalton,

* Hiſtoria Glirium, &c.

and

and of Murray, we are indebted for an inveſtigation of the Count's opinions, the reſult of which ſeems to be, that fluids are not abſolute *non-conductors* of caloric,

Experiments of Dalton, Thomſon, Nicholſon, and Murray, conſidered by the Count.

The experiments of Dalton and Thomſon have proved, that the appearances of currents, ſuch as deſcribed by Rumford, may be often illuſory; and from thoſe of Nicholſon, and from Murray's firſt experiments, we have ſtrong reaſons for ſuppoſing, that the temperature was affected by the conducting power of the fluids employed; but in my opinion the experiments of Murray with a cylinder of ice, are the moſt complete demonſtration of this conteſted point. In a late paper, inſerted in the Tranſactions of the Royal Society of London, Count Rumford endeavours to obviate theſe objections to his hypotheſis, in his uſual ingenious manner.

It is not apprehended that the experiment of Murray could be affected by currents:

Even admitting that in your experiments the caloric was tranſmitted ſolely by the containing veſſel (an opinion by no means probable), and that *currents*, ſuch as Rumford deſcribes, have all the effect he attributes to them in certain caſes; ſtill the experiments of Mr. Murray appear to me incontrovertible. It was not, therefore, without ſurpriſe, that I obſerved him uſe the following argument to invalidate their reſults: "When that veſſel is conſtructed of ice, the flowing "down of the water, reſulting from the thawing of the ice, "will cauſe motions in the liquid, and conſequently inaccu- "racies of ſtill greater moment;" viz. than thoſe produced by the conducting powers of the ſides of the veſſel. Now the melting of the ice could affect the thermometer only by being itſelf heated, and then trickling down the ſides of the cylinder of ice. But I apprehend, the water reſulting from the melting of the ice could not gain a higher temperature than 32° F. while it remained in contact with ice. If we mix even equal parts of water at 172° and ice, we do not find that the temperature of the mixture is above 32°. If ſuch a large quantity of water cannot maintain its temperature in contact with ice, can we ſuppoſe that ſuch a ſmall quantity as was formed could riſe to a higher while trickling down the ſides of a thick cylinder of ice.

and certainly not that with mercury in a veſſel of ice.

But even this explanation of the phenomenon advanced by Count Rumford, is entirely inapplicable to the experiment with mercury; for the drops of water formed could not poſſibly ſink in a fluid ſo much more denſe, nor throw it into currents which could reach the thermometer.

Beſides thoſe moſt ingenious experiments deviſed by Murray, we have other proofs of the conducting power of liquids in ſeveral well known facts. Proofs that fluids are proper conductors.

1*ſt*. If the *non-conducting* power of liquids have any meaning, it muſt ſignify that their particles are incapable of communicating to each other the temperature they have acquired by phyſical contact with ſome other body, whoſe temperature was elevated. If this were true, how ſhall we account for the mean temperature produced by mixing equal quantities of hot and cold water? Rumford, if I recollect aright, has endeavoured to obviate this objection to his hypotheſis by ſuppoſing, that it is only an intimate mixture of hot and cold particles which takes place in ſuch caſes. If this were true, we ſhould expect, from the rapid motion he ſuppoſes the currents to have in liquids that are heated, that they would ſoon ſeparate into warmer and colder ſtrata, from the difference in their ſpecific gravities: This however is not the caſe: The whole acquires a uniform temperature. 1. They take a common temperature on mixing.

2*d*. When mercury and water at different temperatures are mixed, an interchange of caloric takes place. From the very great difference of their ſpecific gravities we cannot ſuppoſe that every particle of the one has been in contact with every particle of the other; yet they ſoon acquire a common temperature, which though not a mean, has always a conſtant relation to the temperature of the two fluids before mixture. Does not this indicate a conſiderable conducting power in thoſe liquids? Indeed, I cannot conceive that any interchange of temperature could take place in ſuch caſes, if the particles of the liquids were incapable of communicating their caloric to the next particles. 2. More particularly water and mercury.

3*rd*. The beautiful experiment deviſed by Rumford, in which water, in a glaſs tube, was made to boil over a cake of ice, by the application of a heated body to the upper part of the containing tube, without, for a very long time, affecting the ice, is a ſufficient proof of the ſlowneſs with which glaſs tranſmits caloric, and clearly indicates that the ſides of the veſſel in ſeveral of the experiments of the above-mentioned philoſophers, could not be the ſole conducting medium. 3. The veſſel is too bad a conductor to account for the effects urged againſt the doctrine of C. R.

4*th*. The ſixteenth experiment in Rumford's ſeventh eſſay, affords another argument againſt his opinion. He poured boiling water on a ſtratum of cold water, which reſted on a cake 4. Hot water poured on cold does not raiſe its temperature by currents.

cake of ice in the bottom of a jar; he found that near the ſurface of the ice the temperature was 40°, at the diſtance of three inches it was 159°, but at the diſtance of ſeven inches it was only 160°. Had the cold water acquired its elevation of temperature by the currents produced, or by the ſides of the veſſel, we ought, I apprehend, to have found the temperature ſpreading more uniformly: but though the firſt four inches only differ by one degree, we find the next three differing by 119 degrees.

5. Heat applied to fluids downwards.

5th. If liquids were abſolute non-conductors of caloric, it would neceſſarily follow, that when caloric was applied to the upper ſurface of different liquids, other circumſtances remaining unchanged, and provided the liquid did not increaſe in ſpecific gravity by cooling, *equal increments of temperature would take place in equal times.*

Apparatus for experiments of tranſmitting heat downwards through fluids.

From ſeveral experiments it is probable, that ſome liquids conduct caloric more rapidly than others. The following were undertaken with a view to aſcertain more accurately this point: How far I have ſucceeded I leave you to judge:

A cylindrical veſſel was turned out of wood, having its ſides 0.5 inch thick; its height four inches, and its diameter two. It has a moveable wooden top or cover perforated with a hole in its centre a little more than an inch in diameter, into which an iron cylinder of one inch in diameter could be eaſily introduced. This cylinder is ſupported by a ſlight flanch or ſhoulder-piece, and can be taken up by means of a ſtring attached to its top. When the iron bar is in its place, its flat lower extremity is 0.5 inch diſtant from the bulb of a delicate mercurial thermometer D E, which is fixed by wax, in a hole perforating the cylinder near its bottom. This thermometer, which was made by the late Ramſden, has a tube as fine as a human hair, and is bended to a right angle, ſo that its bulb and part of its ſtem lie in the axis of the wooden cylinder. This ſhape was preferred, becauſe the ſtem could be little affected by the caloric tranſmitted by the ſides of the veſſel, till after the bulb was acted on by the caloric of the iron bar. A variety of experiments were performed with this apparatus in the following manner: The temperature of the room being ſteadily 67° F. during the trials, a kettle of water was kept boiling over the fire: Its temperature was between 211° and 212°, and into this the cylinder of iron was ſuffered to remain,

Into various liquids in ſucceſſion at 67° F. a cylinder of metal at 212°

main, at each experiment, for 15 minutes. The liquid to be examined, and all the apparatus (but the iron bar), were, at each experiment, aſcertained to be at 67°. The liquid was poured into the wooden veſſel, till it could riſe 0.1 inch on the ſide of the iron cylinder when in its place: The wooden top was put on, and the iron was drawn out of the kettle of boiling water by means of the attached ſtring, and inſtantly let down through the hole of the cover. The time the thermometer took to riſe through three degrees (to 70°) was accurately marked by means of a ſtop-watch, and the reſults of my experiments on ſeveral fluids are exhibited in the following

and the riſe of thermometer 0.5 inch beneath, was obſerved in three minutes.

TABLE.

Table of reſults.

	Liquids.	Minutes.	Seconds.
1.	Water, - - - -	7	5
2.	Milk of a Cow, - -	8	25
3.	Proof Spirit, - - -	8 nearly	
4.	Alkohol. London Pharm.	10	45
5.	Tranſparent Olive Oil, -	9	50.
6.	Mercury, - - -	0	15
7.	Solution of Sulphate of Iron, one part of Salt to five of Water, . - - - -	8	0
8.	Saturated Solution of Sulphate of Alumine, -	9	40
9.	Ditto Solution of Sulphate of Soda, - - -	6	30
10.	Aqua Potaſs. Puræ. Lond. Pharm. - - -	8	15
11.	Saturated Solution of Sulphate of Soda, but the Liquid not touching the Iron Cylinder by 0.1 Inch, or *nearly* $\frac{1}{10}$, -	19	20

As the water in the firſt experiment was employed at a temperature above 42°, it could not affect the thermometer by any change of denſity; it may therefore ſerve as a ſtandard to compare the other liquids. With regard to the differences of a few ſeconds, we need not inſiſt on it as indicating any material difference between the conducting power of the different ſubſtances; becauſe the eye may not be able to mark it inſtantaneouſly: but where this difference amounts to nearly a quarter

The temperature was always too high to produce a deſcending current in water by heating.

a quarter of a minute, much more when to ſeveral minutes, we may fairly conclude, that there is a difference in conducting power.

In all theſe experiments the ſides of the apparatus ſhould have produced equal increments, had this been the cauſe of the riſe of the thermometer; and it is evident that currents downwards could not affect it. That the ſides of the veſſel could not communicate the temperature to the thermometer, nor even the radiant caloric affect it in the manner obſerved, the eleventh experiment (which by the way aroſe from an error in the mode of conducting the trial with ſulphate of ſoda) ſufficiently demonſtrates. From an inſpection of the table, it will be ſeen, that the aqueous ſolutions of different ſalts differ materially from each other in the celerity with which caloric is propagated through them.

I attempted to meaſure the conducting powers of ſeveral of the weaker acids, but I was ſoon convinced that their action on the iron might invalidate the accuracy of the reſults.

The fluids are proper conductors.

It will be unneceſſary to obſerve that if we find the thermometer requiring different times for its elevation, in ſuch caſes, we muſt aſcribe it to the conducting powers of the medium between it and the heated body.

If I am not deceived, we may conclude from what I have above adduced, that liquids as well as ſolids are conductors of caloric; that the tranſmiſſion of it through them follows a particular law depending on the properties of the particular liquid, but which is not in the exact ratio of any of their mechanical properties, though nearer that of their *denſity* than any other.

The Count's facts may be as well explained by the ſlow conducting energy of fluids as by its negation.

Very ſlow currents will explain his fact of the Glaciere of Chamouni.

Such, Sir, are the principal arguments that ſeem to militate againſt Count Rumford's hypotheſis, which he has, with that ingenuity which diſtinguiſhes his reſearches, applied to the ſolution of many important phenomena of nature. Theſe, however, may be equally well explained by ſuppoſing liquids very bad conductors of caloric; and, if the currents cauſed in liquids by changes in temperature, have even a very inferior velocity to what he ſuppoſes, we may, I think, account ſufficiently well for the appearance he obſerved on the Glaciere of Chamouni, which he propoſes as a teſt of his opinions, by the decreaſe in denſity of water while its temperature deſcends from 42° to 32°, (a fact which the Count's late experiments confirm) without aſſenting to his opinions with regard

to

to the non-conducting power of fluids. An examination of this would, however, extend farther this already too long letter; but if you deem such an enquiry interesting, it may be the subject of a future communication.

I am, Sir,
Your's with respect,
T. S. TRAILL.

*** As this letter did not come to hand till above a fortnight after its date, and the verbal description is very clear, it was not thought necessary to postpone it for engraving the author's sketch.—N.

XIII.

Indian Account of a remarkably strong and ferocious Beast, which (they say) existed in the northern Parts of the State of New York about two hundred Years ago. Collected and communicated by Mr.* JOHN HECKEWELDER.

THE jagisho† (or naked animal, or bear, as some of the Indians call it) was an animal much superior in size to the largest bear. It was remarkably long-bodied, broad down its shoulders, but thin, or narrow at its hind legs, or just at the termination of the body. It had a large head and a frightful look. Its legs were short and thick. Its paws (the toes of which were furnished with long nails or claws, nearly as long as an Indian's finger) spread very wide. Except the head, the neck, and the hinder parts of its legs, in all which places the hair was very long, the jagisho was almost naked of hair; on which account the Indians gave it the name of "naked."

Account of the large animal called jagisho by the American Indians.

Several of these animals had before this time been destroyed by the Indians, but this particular one had, from time to time, destroyed many of the Indians, particularly women and children, when they were out in the woods getting nuts, digging roots, &c. or when they were working in the fields. Hunters when fast pursued by this animal, had no means of escaping

* To the Editor of the Philadelphia Medical and Physical Journal, whence this is taken.

† The Indian name of this beast or animal.

from

Account of the large animal called jagiſho by the American Indians.

from it except where a river or lake was at hand, by plunging into the waters, and ſwimming *out*, or *down* the ſtream to a great diſtance, they effected their eſcape. When this was the caſe, and the beaſt was not able to purſue his intended prey any further, he would ſet up ſuch a roaring noiſe, that every Indian who heard it trembled with fear.

This animal preyed upon every beaſt it could lay hold of. It would catch and kill the largeſt bear, and devour it. While the bears were plentiful the Indians had not ſo much cauſe to dread the jagiſho; but when this was not the caſe, he would run about in the woods, ſearching for the track or ſcent of the hunters, and follow them up. The women became ſo much afraid of going out to work, that the men aſſembled to deliberate on a plan for killing him.

This beaſt had its reſidence at or near a lake, from which the water flows in two different ways (or has two different outlets), one northerly and the other ſoutherly. The Indians being well informed of this circumſtance, a reſolute party of them, well armed with bows, arrows, and ſpears, made towards the lake. They ſtationed themſelves on a high perpendicular rock, climbing up the ſame by means of Indian ladders, and then drawing theſe ladders up after them.

After being well fixed, and having taken up with them a number of ſtones, the Indians began to imitate the voices and cries of the various beaſts of the woods, and even thoſe of children, in order to decoy the jagiſho thither. Having ſpent ſome days in this place without ſucceſs, a detached party took an excurſion to ſome diſtance from the rock. Before they had reached the rock again, the beaſt had gotten the ſcent of them, and was in full purſuit of them. They, however, regained that poſition before he arrived. When he came to the rock, he was in great anger, ſprang againſt it with his mouth wide open, grinning and ſeizing upon it, as though he would tear it to pieces * * * *. During this time, numbers of arrows and ſtones were diſcharged at him, until at length he dropped down and expired.

His head was cut off, and was carried in triumph by the Indians to their villages or ſettlements on the North River, and was there fixed upon a pole that it might be ſeen. As the report of the death of the animal ſpread among the neighbouring

neighbouring tribes, numbers of them came to view the head and to praiſe the victorious Indians for their warlike deed.

N. B. The Mahicanni claim the honour of this act.

Account of the large animal called jagiſho by the American Indians.

Remarks by the Editor.

The preceding traditional accounts of the Indians, concerning the "naked beaſt," are in ſome reſpects, ſo very extravagant, that they may perhaps be deemed altogether unworthy of any attention. I muſt confeſs, however, that I cannot but conſider ſuch traditions, though imperfectly handed down to us, and evidently disfigured by fable, as entitled to the notice of the naturaliſt and philoſopher.

That ſuch an animal as the Jagiſho is deſcribed to have been, has ever exiſted in the ſtate of New York, may perhaps admit of a rational doubt; but that the Indian tradition relates to *ſome remarkable* animal that is no longer to be ſeen in the country which it is ſaid to have inhabited, I think there is good reaſon to believe. What this animal was, at what period it ceaſed to be ſeen, and what was the more pure account of the Indians concerning it one hundred years ago, I do not pretend to determine.

Poſſibly the Indian tradition refers to the large animal, (I mean an individul of the ſame ſpecies,) ſome of whoſe bones have been found in a cavern in the back parts of Virginia; the animal of which mention is made in the firſt part of this Journal*. Is is true indeed that the Indian accounts of the activity of the New York animal are not very favourable to the idea, that the animal was Mr. Jefferſon's Megalonyx, which I have ſuppoſed belonged to the order of Tadigrada, comprehending the Sloth, the Armadillo, and others. What is ſaid of the claws of the Jagiſho may be thought to favour the notion that this was really the Megalonyx, or Megatherium. But I would not be underſtood to place any dependance upon the minute or deſcriptive circumſtances which are mentioned in the Indian tradition. Nor indeed do I think it at all probable that the Megalonyx (as it is called) or any of the ſpecies of elephants whoſe exuviæ abound in various parts of North America, have been ſeen in a living ſtate in this Continent, within the period of two, or even twice two hundred years.

* Section Third, p. 152, 154.

SCIENTIFIC NEWS, &c.

Death of Profeſſor Claproth.

Klaproth.

SOME of the foreign Journals have announced the death of the celebrated Klaproth of Berlin, who, for the benefit of the Sciences, continues in good health in his ſixty-ſecond year. Mr. Juſtus Claproth, Profeſſor of Juriſprudence in the Univerſity of Gottingen, well known for ſeveral learned works on that ſubject, died on the 10th February laſt, in his ſeventy-ſeventh year.

Aſtronomical Prize.

Lalande's prize given to Harding.

The medal founded by de Lalande for the beſt aſtronomical work, has been adjudged by the National Inſtitute in its ſitting of April laſt, to Mr. Harding, for his diſcovery of the laſt new planet. That able aſtronomer has been appointed to the direction of the Obſervatory at Gottingen.

New Muſical Inſtrument.

New muſical inſtrument.

A Poliſh clock-maker named Maſlouſky, arrived at Berlin, at the beginning of the preſent year, with the intention of exhibiting a new ſtringed inſtrument, of his invention. Notwithſtanding a variety of advertiſements, he did not ſucceed in attracting the public notice; and he determined to exhibit the inſtrument at a concert previous to his departure. About 300 auditors attracted by the names of Himmel and Seidlen, who were to perform, attended, and towards the end of the concert nearly half the number retired. The artiſt proceeded to exhibit his Kœliſon, which is the name he gives the inſtrument. It conſiſts of a ſound-board, on which the uſual ſyſtem of wires of the piano are fixed. Between theſe wires are ſmall wooden cylinders, which being put into motion, communicate their vibrations to the wires. The tones are ſo ſoft and enchanting that the harmonica cannot equal them, the forte and piano are given in every imaginable gradation, and the whole effect was no leſs ſurpriſing than unexpected, and the maker Huhn received orders for a number of the inſtruments.

The

The preſent article is taken from Millin's *Magaſin Encyclopedique*, who does not ſay whether the wooden cylinders were moved in rotation or otherwiſe, nor how they were applied and preſſed againſt the ſtrings. The leading fact of this notice ſeems to be, that there are certain kinds of wood, and perhaps certain reſinous or other matters to be applied to them, that will produce the effect of a bow upon wire ſtrings in a ſuperior manner. It is indeed probable, that we do not yet poſſeſs much knowledge of the art of producing tones by the powerful expedient of bowing, or light friction; and mechanics have ſtill an ample field for applying this method with force, preciſion, and rapidity to the more compounded inſtruments.

Saverien.

Death of Saverien.

On the 28th of May laſt died Alexander Saverien engineer of the French marine; who has been ſixty years known to the ſcientific world, for his writings on navigation and the theory of building, rigging and manœuvering ſhips. He has written accounts of the inſtruments for making obſervations at ſea; a marine dictionary; a dictionary of the mathematics; a dictionary of architecture; an hiſtory of modern philoſophers, and an hiſtory of the progreſs of the human underſtanding. His works indicate a conſiderable ſhare of talent and very extenſive knowledge. For many of the laſt years of his life he was poor and infirm, and was much indebted to the cares of a ſervant who continued with him from motives of attachment. He died at the age of eighty-five, leaving behind him a widow likewiſe very aged and in want.

Pure and beautiful Ceruſe.

Pure and beautiful ceruſe.

Mr. Van Mons informs us, that, if lead aſhes be diſſolved in a ſufficient quantity of dilute nitric acid by the help of a gentle heat, filtered, and precipitated by chalk in impalpable powder, the precipitate, when waſhed and dried, will be the pureſt and moſt beautiful ceruſe poſſible. The queſtion which offers itſelf on this occaſion, is whether it could be afforded at a reaſonable price.

Chromate

Chromate of Lead and of Silver.

Chromate of lead and of silver diſſolved in nitric acid.

Count Mouſſin Pouſchkin has diſſolved both the red lead ſpar and chromate of ſilver in nitric acid, by adding a little ſugar the moment the acid is poured on, and promoting the action by gentle heat. The ſpar then requires only five or ſix parts of acid, the chromate of ſilver ſtill leſs. Nitrous acid gas is evolved, and the ſolution of the former is an amethyſt colour, of the latter a garnet red, without the leaſt trace of green, either by reflection or refraction.

Putrefaction prevented.

Putrefaction prevented by red precipitate,

Dr. Valli having left a pound of ſoup in which were twelve or fifteen grains of red precipitate, expoſed to the open air for four months, found it exhibited no ſign of putrefaction, and did not even ſeem to have undergone any alteration. He then repeated the experiment for a month in the height of ſummer, with the ſame effect. The oxide in the mean time had neither diminiſhed in weight nor altered its colour.

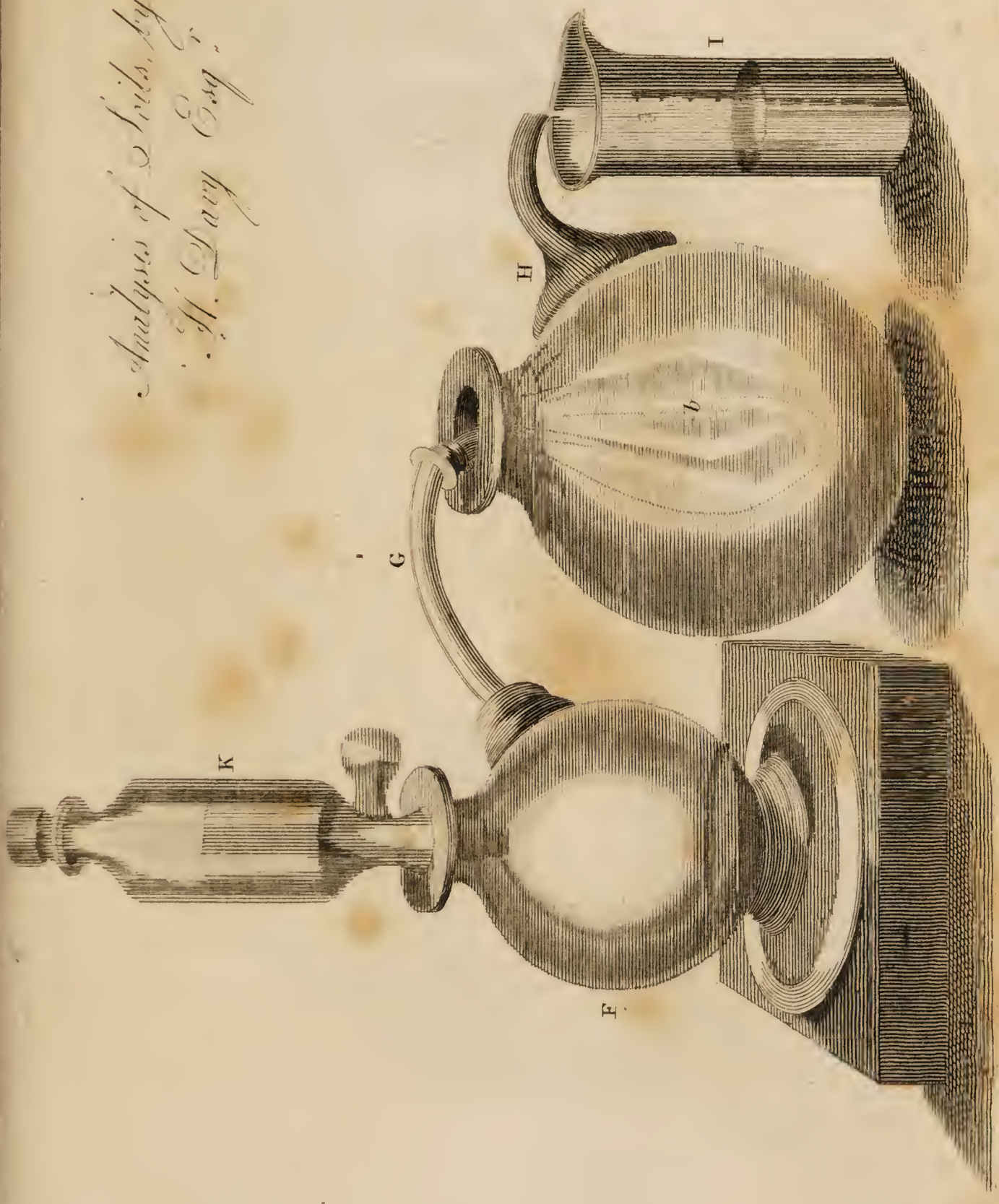
without loſs of weight or colour; and by the combination of oxigen.

On this Mr. Van Mons obſerves, that he has found broth keep for years by means of a few grains of mercury in the ſtate of oxide and citrate. Nitrate of ſilver has long been conſidered as the moſt powerful of antiſeptics, and he has found thoſe of gold and of mercury equally ſo. Oxigenated muriate of potaſh retarded the putrefaction of ſtrong ſoup ſeveral days, and ultimately put a ſtop to it at a certain point. Very dilute nitric acid, and oxigenated muriatic acid in the ſtate uſed for bleaching, preſerved a moderate ſtrong ſoup for ſeveral months.

Leverian Muſeum.

Sale of the Leverian Muſeum.

The Leverian Muſeum which has been near 40 years in collecting at an expence of near 50,000*l.* will be ſold by public auction in May 1806, unleſs an acceptable offer for the purchaſe of the whole be previouſly made. The ſale will take place in the building which now contains the Muſeum. Catalogues are preparing with all ſpeed.



Apparatus of Mr. Gay Lussac for analysing Air by Inflammation.

Nicholson's Philos. Journal. Vol. XII. Pl. VI. p. 144.

Analysis of Soils, by H. Davy Esq.r

Nicholson Sc. Mayfield Ch.r

Fig. 1.

Count Rumford's Investigations concerning heat.

Fig. 2.

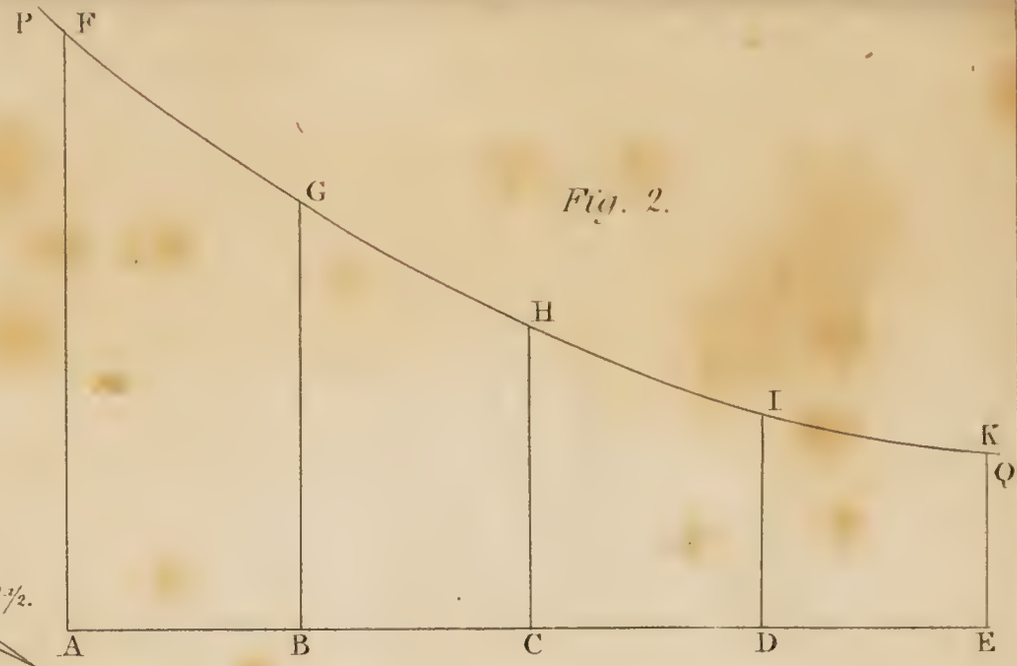


Fig. 3.

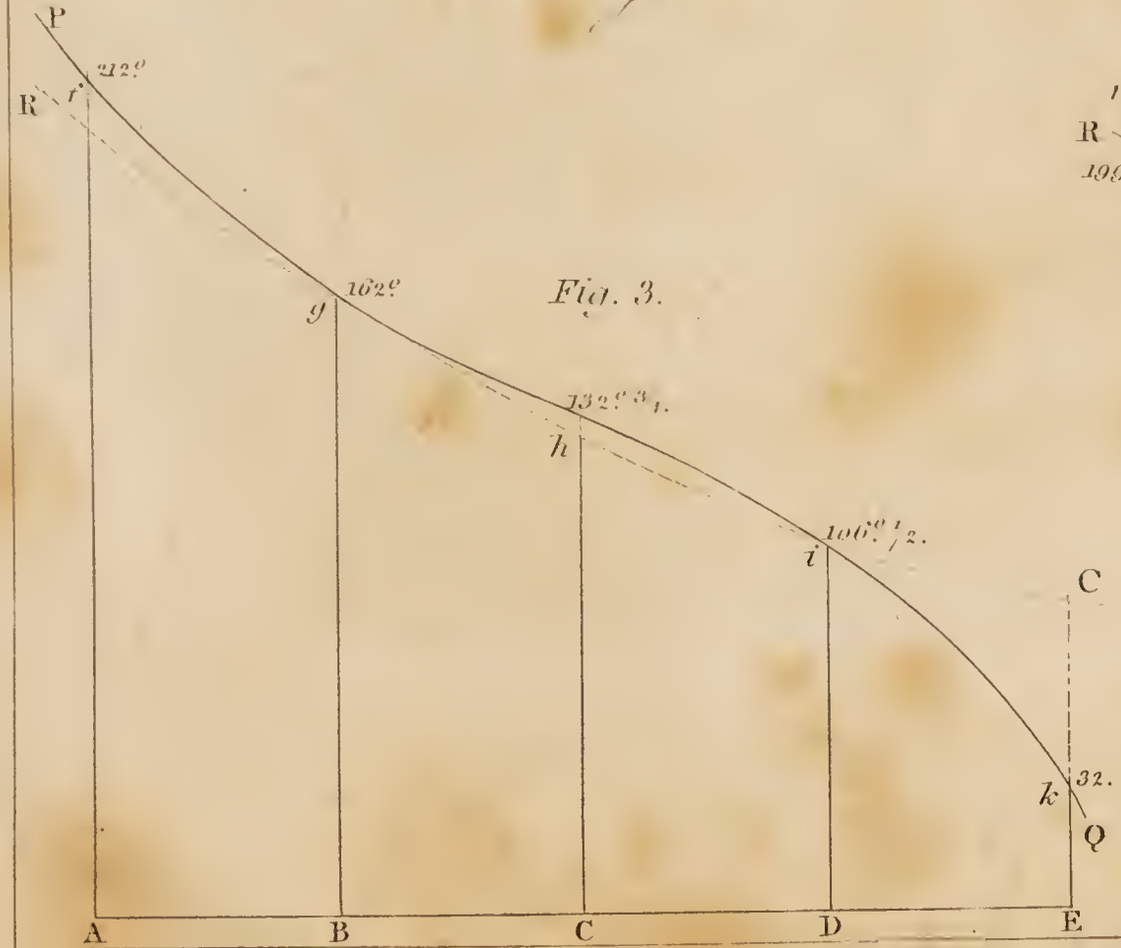
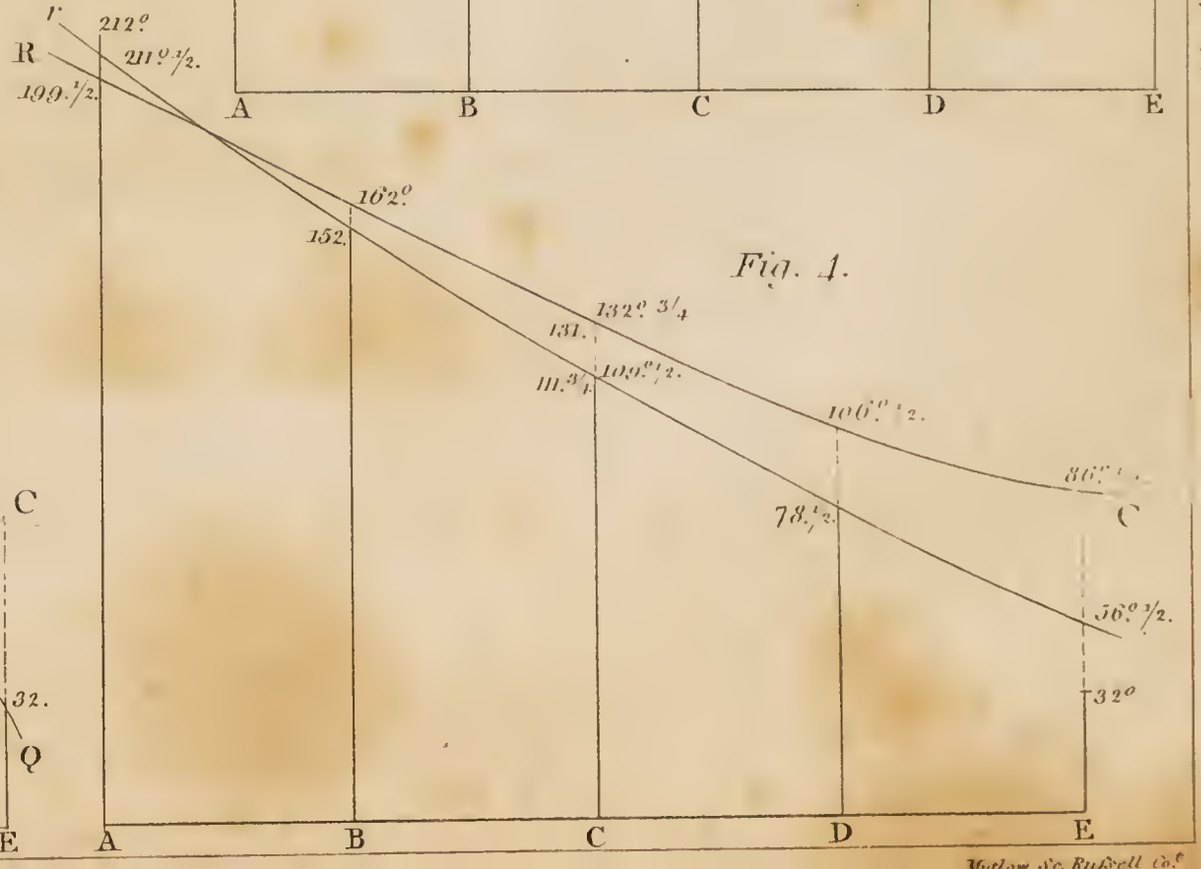


Fig. 4.



Yeates & Rutherford Sc.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

NOVEMBER, 1805.

ARTICLE I.

Facts and Observations relating to the Blight, and other Diseases of Corn. In a Letter from G. CUMBERLAND, *Esq.*

TO MR. NICHOLSON,

SIR, *Weston-supra-Mare, 10th October*, 1805.

ALTHOUGH not much in the habit of taking any thing on trust, I must confess, that when early in the spring the pamphlet of Sir Joseph Banks, called, *A short Account of the Cause of the Disease in Corn, called by the Farmers, the Blight, the Mildew, and the Rust**, came to my knowledge, I felt very much inclined on the credit of his extensive fame to receive with a favourable prejudice, what was so positively announced as information, on a subject that we may all, I suppose, be allowed to have something to do with.

Reference to the Pamphlet of Sir Joseph Banks on the blight in corn.

Having therefore much leisure, and being placed in a favourable situation to make observations, I began doing all that could then be done, viz. planting some wheat near a barberry bush, and searching carefully for the yellow rust on the early leaves.

Corn sowed near a barberry bush.

* Inserted intire in our Journal X. p. 225.

Diſeaſed ſtraw being examined, it was ſeen that Mr. Bauer's drawings are very correct.

I next examined the ſtraw by the microſcope, making many diſſections with great attention. The reſult of which was, a conviction that the deſigners and engravers part of the fine plates that accompany this eſſay, has been excellently and faithfully managed, as far as the then ſeaſon would permit me to compare them, and ſome old ſtraws of the laſt year rendered it very probable, that the whole was correctly drawn.

Doubt whether this diſeaſe affects the grain.

But here I began to ſuſpect, that the *ruſt* was not ſo guilty as has been repreſented, for, admitting for a moment, that it does intercept the ſap by plugging the apertures ſaid to be deſtined in wet weather to receive the humidity of the atmoſphere, yet, as it is *not yet aſcertained* that it ſtrikes root, into the cellular texture beyond the back, for I could not admit of ſaying there is *no doubt* of a thing that *has not yet been traced*, ſee page 11 (or 227 of Journal) and as the plates, if they prove any thing *prove the contrary, vide* fig. 7. I thought we muſt wait awhile before we could charge to this cauſe the diminution of our flour. There was yet another motive in my humble opinion for doubt: I ſaw, even by examining the ſtraws of laſt year, that it was ſcarcely poſſible to find among hundreds of *ruſted ſtraws* that had *blighted heads*, one that any way partook of the ruſt except at the upper joint, and that partially only, while the ſheaths that nature has kindly given to ward off injuries, were compleatly conſumed with it. Now I believe, no one will pretend to aſſert, that this injury done to the ſheaths of the ſtraws could in any way affect the riſing of the *ſap* to the ear; we muſt therefore, I ſoon ſaw, confine the cauſe of injury, *if this be any cauſe*, to the quantity of fungi that more immediately attacks the upper bare joint of the ſtraw.

And even here it appeared at this early ſtage of my doubts to be very uncertain; for if we reflect firſt of all that it is by no means pretended to be proved, that theſe fungi do penetrate the cellular texture; and next, that if they do, it will remain to be proved, that by ſo doing they materially intercept the ſap; and laſtly (which I conceive to be no extravagant conjecture) that if they did, yet as far as the cellular texture of the ſtraw is concerned in conveying it, the interception could only be very partial.—Taking all theſe reflections together, I think I was grounded in entertaining doubts of the true *cauſe* having been exhibited, as is ſet forth in this pamphlet, p. 13.

p. 13. (or 228 Journal.) Where the preſident ſays, "though diligent enquiry was made during the laſt autumn, no information of importance relative to the origin or the progreſs of the *blight* could be obtained! this is not to be wondered at, for as no one of the perſons applied to had any knowledge of the *real cauſe* of the malady, none of them could direct their curioſity to the real channel. *Now that its nature and cauſe have been explained*, we may reaſonably expect that a few years will produce an intereſting collection of facts and obſervations, and we may hope that ſome progreſs will be made towards the very deſirable attainment of either a preventive or a cure." page 14.

Country names of the diſeaſes of wheat.

Having thus advanced in the examination as far as the ſeaſon would afford, I thought it would be beſt to aſcertain in the county in which I happened to be placed, the terms (intirely overlooked in the pamphlet) there applied to the different diſeaſes of corn; and here it ſoon appeared that the terms are not univerſal.

First blight; or mildew, by which the embrio grain is turned to powder.

The firſt blight (for there are many) is that early appearance of intirely decayed ears, of plants apparently in a healthy ſtate, but which, in the embrio which lays within the upper ſheath, before the ear is develloped, has turned to a brown puce-coloured powder. This by ſome has been ſuppoſed to originate from defective ſeed, but ſurely improperly; for many of theſe ears are found to be the fineſt and largeſt in the field in their embrio ſtate; and to me it ſeems evident, that they are imperfect from a really defective parturition, owing to ſome accidental circumſtance; or poſſibly (for they generally ſtand below the others) from the want of ſun to uncloſe the upper ſheath at a critical moment.

The ears are uſually crooked at bottom, &c.

Theſe ears are alſo in general found to be crooked at the root of the ear ſtalk, owing to the effort to raiſe the ear acting within the ſheath on a decayed and mouldy ear; but it appears that they are all at length ejected from the ſheath, and the brown duſt blown compleatly away, ſo that the ſtalk at laſt remains without a ſingle grain on it, ſtanding up like a bare and barren pole. In this manner many ears at firſt periſh, but the quantity is ſeldom any object to the farmer.

Appearance of the powder; it produces no infection.

Of the puce-coloured powder a quantity was collected: it had no ſmell, but felt ſoft to the touch, like whiting, though more greaſy. An attempt was made to innoculate a number

of other ſound ears with it; by rubbing it on the leaves, by giving friction to the ſtraw itſelf with it, by inſerting it into the ears, by placing it beneath the ſheaths, and laſtly, by introducing it into the pith of ſeveral ſtraws,—each of theſe in wet and dry weather, but nothing took any effect. It fell harmleſs:—but one diſcovery aroſe from examining the ears in which it was produced, viz. that corn is capable of being completely blighted without any external diſeaſe or application. For all the ſtraws were without blemiſh as well as the leaves, and conſequently we have no occaſion to recur to external cauſes for this internal decompoſition.

The firſt blight does not ariſe from an external cauſe.

Wheat in this ſtate I drew very accurately, and ſent ſpecimens of it to town in this condition to Mr. Nicholſon; and thus ended the firſt blight as it is called in Somerſetſhire, but which might poſſibly be with more propriety termed the *mildew*.

Second diſeaſe. Smut. Full grains, containing only a dark powder of a fiſhy ſmell.

The next diſeaſe appears in thoſe ears which ſtand erect and ſtaring, indicating their lightneſs by their attitude. Although on gathering they appear full of corn, they turn out in effect to be full of a dark powder that has the ſmell of ſtale lobſters or ſhrimps, when preſſed between the fingers. This powder on examination by the microſcope ſhews ſome ſaccharine concretions among it, but it has none of the actual properties of wheat. Upon carefully examining theſe grains the outer ſkin was diſcovered to be *intire, unperforated, perfectly green, and perfectly full*;—yet ſtrange to tell, if it be not really the work of inſects, one half of an ear was often found to be thus ſmutty while the other half was ſound.

Mechanical injury did not produce this diſeaſe.

Among other conjectures, it was thought that this ſmutty diſeaſe might ariſe from the juices of the ſtraw being intercepted by accidents. In order therefore to try what could be effected by injuries done to the ſap, I bound ſome ears, wounded others by preſſure, divided ſome with a knife near the ſtem, marking each by cutting off the beards with ſciſſars; yet I never found any ſuch effect produced, as either ſmut, or even decay, and all the ears thus injured came very well to maturity. Here were real injuries, and committed at a time (June) when the ears were by no means far advanced, and this led me ſtill leſs to expect any great effect from a little partial moſs adhering to the ſtem, the accidental effects of the ſeaſon at a later period.

At length on the 1ſt of Auguſt, I ſaw in a low field the firſt appearance of yellow ruſt, but collected one with difficulty; and now I found that it was univerſally agreed by our farmers, that this fungus, as our plate deſcribes it, was conſidered as the effect of fogs or great humidity, which firſt attacks the leaf or ſheath of the ſtraw, but ultimately penetrates, and vegetates on the upper joint of the ſtraw itſelf, where it is uncovered juſt before the corn is ripe, ſo that what our plate exhibits is by all agreed to be called the *ruſt* in all ſtages; but that the ruſt cauſes the *latter blight*, or ears with ſhrivelled grains does not ſeem to be ſo generally agreed.

Third diſeaſe; the ruſt conſidered as effect of humidity.

To prevent the *ſmut*, our farmers ſteep their wheat in ſalt water, in order to ſeparate the ſound grains from the light or blighted better than by freſh water, becauſe ſalt water naturally floats all but the heavieſt. In the *Venetian* ſtate I remember they added ſaltpetre to the ſteep on the ſame principle, a very different ſyſtem from that of Sir Joſeph, who recommends, I think very dangerouſly, the uſe even of tailings as ſeed, and this on mere hot houſe experience, ſee p. 25 (or 232 Journal.)

The proceſs of ſteeping grain is uſed to ſeparate the heavieſt for ſeed.

An old and good farmer * laſt year at *Weſton*, bought good wheat for ſeed becauſe his own was *ſmutty*, but not having quite enough, he ſowed about three pecks of his own ſmutty wheat to finiſh, and it turned out quite as good wheat as ſome of that which was bought as the beſt; yet this does not confirm the doctrine, I think.

Smutty grain not uſed for ſeed, yet ſome ſucceeded.

Having thus aſcertained what is called the early *blight or mildew*, together with that which follows, and is known by the term *ſmut*, and alſo the diſeaſe which comes next, and being ſimilar to Sir Joſeph's Banks's plate, is called *ruſt*, and laſtly the *latter blight* which is ſeen in merely ſhrivelled grains, or grains imperfectly ripened;—I ſhall now proceed to the ſpecimens which I gathered in all the ſtates, made drawings of them, and ſtill retain in the ear labelled with great care, as proofs of what is here advanced; after which I ſhall make ſome deductions.

Latter blight; or ſhrivelled grains.

On 22nd of July, 1805. I began my collection; and No. 1. contains healthy ears, clean to outward appearance, the loweſt leaf a ſpeck or two of fungus. Second joint from the head a little reddiſh. No bloom on the cane.

Accounts of ſpecimens of wheat gathered in this enquiry.

* Mr. Oakley of Weſton-Supra-Mare.

Accounts of ſpecimens of wheat gathered in this enquiry.

No. 2. Much diſeaſed ſtraw, bloom or mould under the ſheath, and on the leaf; in other reſpects the ſtraw quite as healthy and ſound as No. 1.

No. 3. Entirely diſeaſed, yet every grain full and of its proper ſize—ſome grains evidently opened at their ſides by ſome ſmall inſect. The outſide of this ear quite green and healthy, not even a ſpot, ſmell of the ear very fiſhy, not wanting one grain. The head ſtalk not even waved at the ear, root intire, upper ſtalk a little yellow.

No. 4. A healthy ear of the bearded, thirteen rows of grains, ſtraw mouldy under the ſheath, and at joints, yet ſound, laſt or upper joint green.

No. 5. Another nearly ſimilar.

No. 6. A double ſtraw to one root, both ears perfect, both ſtraight at head ſtalk, both diſeaſed, yet full and plump; ſome grains ſound yet green, and cloſe to a black one; a rich golden coloured moſs or duſt at the back of the green coat, yet the grain coat perfectly green and uninjured, while the grain was compleatly full of the black fiſhy-ſmelling blight inſtead of flour. Straws green at the top and quite healthy throughout.

No. 7. A ſtraw that having been blighted in the ſheath by the early blight, had thrown off all its brown duſt, and grown to a ſtrong ſtraw: The ſkeleton of the ear only remaining, very crooked near the ear: The upper ſtalk evidently by its purple ſtripes diſeaſed under the ſkin, yet no moſs or fungus protruded on the cane; ſtripping back the firſt leaf from the head, I found the powder from the ear had adhered to the ſtraw under the ſheath, and that it was mouldy: The ſecond joint quite healthy under the leaf, but with the red and purple ſtreaks where uncovered to the light; The third joint the ſame: The ſheath leaves themſelves healthy.

No. 8. Spotted ear externally, ſound ſtraw, yet having black duſt at the joints, in this ear I found a maggot about the 30th part of an inch long, of the exact yellow of the powder found behind the diſeaſed grains, (that powder may be his excrement) viz. orange yellow, his form reſembled the maggot of nuts, lived an hour on the table.

No. 9. Diſeaſed ear, grains all blighted yet no yellow powder under the ſheaths of the grains, ſtraw healthy, ear fiſhy ſmell.

No. 10.

No. 10. Ditto with an inſect very active in the ear, yellow, ſee drawing A B. Fig. 1. Plate IX.

No. 11. Two perfectly healthy ears from the ſame root, both healthy throughout, yet on the leaf a ſpeck or two of Sir Joſeph's fungus was originated, and well grown: it was of the orange colour.

No. 12. An ear faſt ripening, ſolid in the grain, yet had loſt its firſt ſix lower grains, laſt joint green, beards yellow, a few ſpots on the ſheath of the grain, ſeemingly occaſioned by a ſmall black fly found in it.

Obſervations made on the ſame Day on Grains.

Diſeaſed grains were always found to be full and plump without any aperture; the fine ſkin that holds the flour very green, yet all black within; the external ſurface of the black matter covered with a white concretion, perhaps the ſaccharine matter of the wheat. Sound grains found in diſeaſed ears, all the diſeaſed ears ſmelt fiſhy. Diſeaſed grains deſcribed.

Obſervations made on the Straw.

The diſeaſe always attacks the portion of the ſtraw that peeps beyond the ſheath leaf near the joint, and evidently commences at the pores, as old ſtraws will ſhew; but the fungus cannot, I think, be the cauſe of the diſeaſe; becauſe where no fungi have taken root, the corn is compleatly corrupted. Theſe fungi grow it is true, faſteſt on diſeaſed ears, probably becauſe when the ear is diſeaſed it draws leſs humidity from the ſtraw, ſo that the diſeaſed ears ſeem to aſſiſt the growth of the fungi, not the fungi the diſeaſe. In fact, I believe they live on the ſuperfluous moiſture of the ſtraw, or returning ſap. Diſeaſed ſtraw.

On the 27th July laſt, 1805. I examined at leaſt twenty blighted ears of corn, and could only find one among them that had the ſmalleſt degree of *ruſt*, and that only a ſpeck or two on the lower leaf that ſheaths the third joint. The ſpots were orange colour and deep purple, and did not occupy half the diameter of a ſpace of three inches long. Blighted ears, the ſtraw not ruſted.

Upon again examining many *healthy plants*, I found not only their ſheath leaves, but even the ſtraw eaten away by Healthy plants having the ſtraw injured.

ſome

ſome inſect, and at the ſame time diſcovered abundance of the green locuſt like inſect on the ears; of which, ſee the magnified deſign, Fig. 2. Plate IX.

Inoculated plants, and others injured by violence, took no diſeaſe. Plants gathered which have the ſtraw and the grain not in the ſame (healthy or diſeaſed) ſtate.

Some plants that I had lately innoculated were ſtill ſound: others that I had pinched and bruiſed in the upper joint ſhewed no alteration, but in all reſpects reſembled the moſt healthy.

Aug 3rd, 1805, I went to a field of Mr. Oakley's, o Weſton, where the wheat had a general good aſpect; and ſelected and labelled, of ears green yet full.

No. 1. Sound ears with diſeaſed ſtraws.

No. 2. Compleatly ſmutty ears with ſound ſtraws in every reſpect.

No. 3. A ſmutty and a ſound ear from the ſame root. The ſound ear had a ſpeck of fungus.

No. 4. Ears with crooked tops, others twiſted by ſpiders; others with crooked beards, ſhort ſtalks and long; yet all of them full of grain, green and ſound.

No. 5. Ears half ſmutty, viz. on one ſide all the way up.

No. 6. Ears half ſtagged, (ſtagged means here thoſe that ſhew only the ſkeleton of the ear and the crooked upper ſtalk from early blight, all but the bare poles being blown away) theſe ears were half *ſtagged* and half covered with grains.

No. 7. Bare *ſtaggs*, but with quite fine ſound ſtraw.

Other inſtances of ſound wheat with ruſty ſtraw; and ſhrivelled wheat with ſound ſtraw.

Laſtly; in Auguſt and September, 1805, at Allcombe near Minehead, I collected out of a field juſt reaped, two bundles which with all the others I ſtill keep as proofs of my aſſertions: One all of ſound wheat with all their upper ſtalks very much covered with the ruſt of Sir Joſeph Bank's deſcription, and the other all of black and ſhrivelled ears, yet all ſound in the upper ſtalk.

The ſhrivelled corn was never fairly ripened.

Theſe latter mentioned ſtalks, I think, throw great light on my ultimate conjectures drawn from every obſervation through the whole ſeaſon; viz. they preſent ſhrivelled, blighted grains, and exhibit ſhort ears, becauſe on examination they were evidently never ſufficiently expoſed to light and heat: for all their ſtraws though clean were green, not yellow as thoſe of ripe wheat ought to be, and their ſmoaky miſerable appearance (not having the leaſt ſmell of the fiſhy ſmutt) could only ariſe from their humble ſituation below the other ears, where air, ſun, and light was deficient; in fact, they never ripened properly. The ſtraw remained green, and the ſap

probably

probably returned inſtead of being intercepted by the drying of the upper joint; and to me it now appears to be a fair conjecture, that what is generally called blighted corn, or thoſe ears that produce ſo many ſhrivelled grains, and which we are called upon to ſeek a remedy for, is nothing more than the effect of a practice of late much recommended, viz. *to reap early*, a practice not only promoted by the *Miller*, who is eager for the new corn to come to market, but by the avarice of the *Farmer*, who fears that by letting it ſtand too long the grain may fall in price, and reduce his profits; and, what is ſtill more unfortunate, by ſome agricultural writers of great reputation, who recommend it as ſtopping the progreſs of the ruſt, forgetting that the ſun only can effectually deſtroy that ſuppoſed evil, by well drying the ſtraw.

What is called blighted corn ſeems to be corn prematurely reaped, from avarice or ignorance.

Far be it from the writer of theſe few remarks to diſcourage any attempt at ſaving in a remarkably wet ſeaſon, or in remarkably wet ſituations, wheat that has paſſed the period uſually productive of ripe corn. He knows that in caſes of laid-wheat in ſhady ſituations, by reaping it early, we may accelerate the ripening of that which otherwiſe would not have ripened at all, by the operation of turning and expoſing the ſheaves to the ſun, and ſo make good ſaving crops; but what he wiſhes to guard againſt is, that eagerneſs for putting in the ſickle originating in the motives before alledged; for, reaſoning from analogy do we not always find, that in all other ſeeds that are to be gathered, thoſe alone are plump, ſound, and full of their proper flour, that are ſuffered to receive the utmoſt influence of the ſun while on the ſtalk; and he always thought many years ago, that we were in the habit of being too fearful of the latter ſeaſons; it is true that late harveſts are expenſive in collecting, but they are generally well matured, and the inſtance of *barley* that may be well ſaved (for colour can have little to do with the intrinſic value of grain) even as late as November, proves the juſtice of the obſervation; fruit gathered too ſoon, diſappoints all views of profit or pleaſure, and we might, he thinks, as well attribute the ſhrivelling of our apples early gathered to the influence of the apple-tree moſs, as the ſhrivelling of our grains of wheat to a ſuppoſed blight originating in the funguſes, that have of late ſo much alarmed the theoretical agriculturiſts, and œconomiſts of our day.

It is often adviſeable to reap early.

Argument in favour of late reaping.

On

Conclusion.

On the facts expoſed I could greatly enlarge, but I think on all accounts it is beſt at preſent to offer them in their preſent form to the reaſoning faculties of your readers. At any rate they may ſerve as data, and if they ſhould fail to bring others to my opinion, may act, I truſt, as uſeful ſtimulants to the further inveſtigation of a very curious ſubject of enquiry, as to what are the nature of the enemies to the perfection of our wheat harveſt.

With reſpect and eſteem,

I am, Sir,

Your moſt obedient humble Servant,

G. CUMBERLAND.

The vicinity of the barberry does not ſeem to affect grain.

P. S. I ought to obſerve, that on the grains of wheat ſown near the barberry, I had no opportunity of making obſervations; but that I have a dried root of wheat now by me, on which there are above 100 ſtraws that are all clean and ſound, though it grew a few years ago in a garden where barberry buſhes were.

II.

Experimental Inveſtigations concerning Heat. *By* BENJAMIN COUNT OF RUMFORD, *V. P. R. S. &c.*

(Concluded from Page 75.)

SECT. III. *Experiments tending to ſhew that Heat is communicated through ſolid Bodies, by a Law which is the ſame as that which would enſue from Radiation between the Particles.*

Object of inquiry; the laws of the propagation of heat in ſolids.

HAVING made a conſiderable number of experiments on the paſſage of heat through fluids, and through different ſubſtances in the ſtate of powder, I was curious to aſcertain the laws of its propagation through ſolid bodies, particularly metals.

I hoped this diſcovery would furniſh ſome additional data, to confirm or refute the opinions I had adopted concerning heat and its manner of acting; and it will be ſeen by the reſults, that my expectations were not fruſtrated.

Having

Having procured two cylindrical veſſels of tin, each ſix inches in diameter and ſix inches high, I faſtened them together by means of a ſolid cylinder of copper ſix inches long and an inch and half in diameter, which was fixed horizontally between the two tin veſſels. The extremities of the cylinder paſſed through two holes an inch and half in diameter, made for the purpoſe in the ſides of the veſſels, midway between the bottom and top, and were ſoldered faſt in them. Deſcription of an inſtrument: Two cylindrical tin veſſels were connected by a bar of copper.

Each of the veſſels was made flat on the ſide where the copper cylinder was faſtened, ſo that the extremity of the cylinder did not project into the veſſel, but was level with the flattened part.

This inſtrument was ſupported at the height of eight inches and half above the table on which it ſtood, by means of three feet, two fixed to one of the veſſels, and one to the other.

One of theſe veſſels being filled with boiling water, the other with water at the freezing point; as the two extremities of the cylinder were placed in immediate contact with theſe two maſſes of fluid, a change of temperature muſt neceſſarily take place by degrees in all the interior parts of the cylinder. For the purpoſe of obſerving this change, three vertical holes were made in the cylinder, into which were introduced the bulbs of three ſmall mercurial thermometers. One of the holes was in the middle of the cylinder; the others midway between the centre and either extremity. The veſſels filled with water, one at 212°, the other at 32°. The changes marked by three thermometers at equal diſtances.

Each of theſe holes is four lines in diameter, and eleven lines and half deep; ſo that the bulbs of the thermometers, which are three lines in diameter, were all in the axis of the cylinder.

When the thermometers were put in their places, the holes were filled with mercury, in order to facilitate the communication of heat from the metal to the bulb of the thermometer.

To keep the hot water conſtantly boiling, a ſpirit lamp was placed beneath the veſſel containing it; and to keep the cold water conſtantly at the temperature of melting ice, freſh portions of ice were added to it from time to time. The one water kept boiling by a lamp; and the other cold by addition of ice.

The thermometers are graduated to Fahrenheit's ſcale, the freezing point being marked 32°, and that of boiling water 212°.

As the firſt and moſt important object I had in view was, to learn at what temperature the three thermometers would become The thermometers not noticed till nearly ſtationary.

come ſtationary, I did not very carefully notice the progreſs of the thermometers toward this point; but as ſoon as they appeared nearly ſtationary, I obſerved them with the greateſt attention for near half an hour.

The thermometers diſtinguiſhed.

To diſtinguiſh the three thermometers I ſhall call that neareſt the boiling water B, that in the centre C, and that neareſt the cold water D.

Experiment.

The following are the progreſs and reſults of an experiment made the 28th of April, 1804, the temperature of the air being 78° of Fahrenheit.

Tabulated reſults.

Time.	Temperature of the hot water.	Temperature marked by the thermometer B.	Temperature marked by the thermometer C.	Temperature marked by the thermometer D.	Temperature of the cold water.
H. m. s.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.
1 52 15	212	160	130	105	32
— 53 30	—	160½	131	105½	—
— 55	—	161	131¾	106	—
— 56 30	—	161¾	132	106½	—
— 58	—	162	132½	107	—
2 0 0	—	162	132¾	107½	—
— 1 30	—	162	133	107½	—
— 4	—	162	132¼	106½	—
— 6	—	162	132	106	—
— 9	—	162	132¼	106½	—
— 11	—	162	132¾	106½	—
— 28	—	162	132¾	106½	—

Account of the reſults that would have followed the hypotheſis of heat being propagated through bodies by radiation from particle to particle.

Before I proceed to examine more minutely the reſults of this experiment, I will endeavour to ſhow thoſe reſults which it ought to have exhibited, on the ſuppoſition that heat is propagated, even in the interior of ſolid bodies, by *radiations* emanating from the ſurfaces of the particles compoſing theſe bodies.

The particles of ſolid bodies are diſtant from each other.

On this ſuppoſition we muſt neceſſarily conſider the particles that compoſe bodies as being *ſeparate from each other*, and even to pretty conſiderable diſtances compared with the diameters of theſe particles; but there is nothing repugnant to the admiſſion of this ſuppoſition; on the contrary, there are many phenomena which apparently indicate that all the ſolid bodies with which we are acquainted are thus formed.

To

To ſee now by what law heat would be propagated in a ſolid cylinder, let us repreſent the axis of this cylinder by a right line A E, *Plate* VII. *Fig.* 1; (ſee our laſt number) and let us begin with ſuppoſing that the cylinder conſiſts of three particles of matter only, A C E, placed at equal diſtances in that line.

If the ſolid cylinder conſiſt of three equi-diſtant particles,

Let us farther ſuppoſe, that the extremity A of the cylinder is conſtantly at the temperature of boiling water, while its other extremity, E, remains invariably at the freezing point.

—and the extremes at the boiling and freezing temperatures.

By an experiment, of which I have already given an account to the claſs*, I found that when two equal bodies, A B, one hotter than the other, are iſolated and placed oppoſite each other, the intenſities of their radiations are ſuch, that a third body, C, placed in the middle of the ſpace that ſeparates them, will acquire a temperature by the ſimultaneous action of theſe radiations, which will be an arithmetical mean between thoſe of the two bodies A and B.

Then the middle particle would have the arith. mean temperature: A body midway between two others acquires the mean heat by radiation,

From the reſult of this experiment we have ground to conclude, that if the cylinder were compoſed of three particles of matter only, A, C, E, the particle C, which is in the middle of the cylinder, muſt neceſſarily have the arithmetical mean temperature between that of A and that of E, which are at the two extremities of the cylinder; that is to ſay, between 212° and 32° of Fahrenheit, which is 122°.

—or 122°.

Now let us interpoſe between the particles A, C, and E, two other particles B D, and ſee whether the introduction of theſe two particles will make any change in the temperature of the particle C that occupies the middle of the cylinder.

Suppoſe two more particles to be interpoſed.

If the particle B be placed in the middle of the ſpace compriſed between the extremity A of the cylinder and its middle, C, it ought to acquire a mean temperature between that of the extremity A of the cylinder, and that of the point C, namely that of 167°, the mean between 212° and 122°; and if the particle D be placed in the midſt of the ſpace compriſed between the middle of the cylinder and its other extremity, E, this particle ought to acquire a mean temperature between that of the middle of the cylinder and that of its extremity E; it ought then to have the temperature of 77°.

Theſe would each take the mean temperature between the middle particle and the neareſt end particle,

From this new arrangement, the particle C, ſituate in the middle of the cylinder, will find for its neighbours on one ſide

—and therefore they would not alter the temperature,

* See our Journal, IX. 193.

the

the particle B, at the temperature of 167°, and on the other the particle D, at that of 77°. The point in queſtion is, whether the preſence of theſe two particles will make any change in the temperature of the particle C, or not.

—of that middle particle.

In the firſt place it is evident, that if the caloriſic influences of the particle B on the particle C be as efficacious in heating it, as the frigoriſic influences of the particle D be in cooling it, the temperature of the particle C ought not to be changed. But experience has ſhewn, that, at equal diſtances and equal intervals of temperature, the caloriſic influences of hot bodies, and the frigoriſic influences of cold bodies, are exactly equal; and as the diſtance from B to C is equal to the diſtance from D to C, while the interval of temperature between B and C = 45°, is the ſame as that between D and C = 45°; it is evident that the temperature of the particle C, which is in the middle of the cylinder, can be no way affected by the introduction of the intermediate particles B and D.

And by the ſame reaſon it would not be changed by other particles interpoſed.

By the ſame way of reaſoning may be proved, that the introduction of an indefinite number of intermediate particles would produce no change in the temperature of the middle of the axis of the cylinder, or in any part of it; and if the introduction of an indefinite number of intermediate particles make no change in the ſtate of a thermometer placed in the middle of the axis of the cylinder, we may conclude that the thermometer would remain equally ſtationary, if the number of intermediate particles were increaſed till they had that proximity to each other which is neceſſary to conſtitute a ſolid body. If, inſtead of a ſingle row of particles in a right line, there were a bundle compoſed of an indefinite number of ſuch rows placed ſide by ſide, forming a ſolid cylinder, the temperature in the different parts of the line A E would remain the ſame.

But the temperature of a continued ſolid ſh uld decreaſe from one particle to another in arithmetical progreſſion. This is true only when the ſolid is remote from other bodies.

From this reaſoning we may infer, that the temperatures of the different parts of the cylinder ſhould decreaſe in arithmetical progreſſion from one extremity of the cylinder to the other.

But it is evident, that this law of decrement of temperature could take place only in the ſingle caſe of the ſurface of the cylinder being completely iſolated, ſo as to be no way affected by the action of ſurrounding bodies, which is abſolutely impoſſible.

Our experiments are always thus influenced.

The circumſtances under which the experiments were made are very different from thoſe here taken for granted. The

bodies

bodies we ſubject to experiment are conſtantly ſurrounded on all ſides by the air and other bodies which act on air inſtruments continually, and often in a very perceptible manner; and we can never hope to iſolate a cylinder ſo completely that the apparent progreſs of heat in its interior ſhall perceptibly obey the law we have juſt diſcovered. In common caſes it deviates widely from this law.

As the cauſes of this deviation are well known, we will ſee whether there be no means of appreciating their effects. Appreciation of the effect.

The ſurface of the cylinder being ſurrounded by the atmoſpheric air and other bodies, all which are of a known and ſenſibly conſtant temperature, we may determine the comparative effects of theſe bodies on the different parts of the ſurface of the cylinder. The atmoſphere will affect the cylinder,

In thoſe parts of the cylinder which are hotter than the air and other ſurrounding bodies, the ſurface of the cylinder will be cooled by the action of theſe bodies; but if one of the extremities of the cylinder be colder than the atmoſpheric air, thoſe parts of the cylinder which are colder than the circumambient fluid will be *heated* by its influence and that of the ſurrounding bodies. ——by cooling its hot part and heating its cold part.

We will begin with examining the caſe where the coldeſt extremity of the cylinder is at the ſame temperature as the ſurrounding air. Let us ſuppoſe then, that the experiment with boiling water at the one end and freezing at the other be made when the temperature of the air is at the freezing point, or 32° of Fahrenheit. If one extremity of the ſolid be at the temperature of the air, the other hotter,

In this caſe it is evident that the ſurface of the cylinder muſt every where be *cooled* by the influence of the ſurrounding atmoſphere. The queſtion then is to determine the comparative effects, or the relative quantities of refrigeration or loſs of heat, that muſt take place *in the different parts of the cylinder*: and in the firſt place it is clear, that the hotter a given part of the cylinder is, the more heat it muſt loſe in a given time, by the influence of the ſurrounding cold bodies; whence we may conclude, that the refrigeration of the ſurface of the cylinder by the influence of the air and other ſurrounding cold bodies muſt neceſſarily diminiſh from the extremity of the cylinder A, which is in contact with the hot water, to its extremity E, which is in contact with the cold, ——the ſurface will be every where cooled, —but moſt ſo where the heat is greateſt.

From

The change is in proportion to the difference of temperature.

From reaſoning which appears incontrovertible, and which the reſults of a great number of experiments appear to confirm, it has been concluded that the celerity with which a hot body placed in a cold medium is cooled, is always proportional to the difference between the temperature of the hot body and that of the medium. Conſidering this concluſion as eſtabliſhed, we may determine *a priori* what ought to be the gradation of temperatures in the interior of a given ſolid cylinder ſurrounded by air, one extremity of which is in contact with a conſiderable body of boiling water, while the other is ſimilarly in contact with cold.

We have ſeen that, if the ſurface of the cylinder were perfectly iſolated, the decreaſe of temperature from the hotteſt extremity of the cylinder A to its other extremity E, which is in contact with cold water would be *in arithmetical progreſſion*, and it has juſt been ſhewn, that the decreaſe muſt neceſſarily be accelerated by the action of the air and other ſurrounding cold bodies.

But the acceleration of the decreaſe of temperature in thoſe parts of the cylinder which are toward the cold extremity, depending on the action of the air and ſurrounding bodies, muſt be continually diminiſhing in proportion as the temperature of the ſurface of the cylinder approaches nearer and nearer that of the air; and hence we may conclude, that if a given number of points at equal diſtances from each other, be taken in the axis of the cylinder, the temperatures correſponding with theſe points will be *in geometrical progreſſion*.

Whence the temperatures will be in geometrical progreſſion,

We may repreſent the progreſs of the decreaſe of temperature by *Fig. 2. Pl.* VII.

—and may be repreſented by the logarithmic curve. Figure conſtructed.

In a right line A E, repreſenting the axis of the cylinder, if we take the three points B, C and D, ſo that the diſtances A B, B C, C D, and D E ſhall be equal; and, erecting the perpendiculars A F, B G, C H, D I, E K, take A F = the temperature of the cylinder at its extremity A, B G = its temperature at the point B, and ſo of the reſt; the ordinates A F, B G, &c. will be in geometrical progreſſion, while their correſponding abſciſſes are in arithmetical progreſſion; conſequently the curve P Q, which touches the extremities of all theſe ordinates muſt neceſſarily be the *logarithmic* curve.

Compariſon of the theory with the experiment.

We will now ſee, whether the reſults of experiment agree with the theory here exhibited or not.

To

To form our judgment with eaſe, and as it were at a ſingle glance, of the agreement of our theory with the reſults of the experiment, of which I gave an account at the beginning of this memoir, we have only to repreſent theſe reſults by a figure in the following manner.

Conſtruction of a figure exhibiting by a curve, and its ordinates the temperatures actually obſerved.

On the horizontal line A E, *Fig.* 3. repreſenting the axis of the cylinder employed in the experiment, we will take three points, B, C and D; one, C, in the middle of the axis, being the ſituation of the central thermometer, the other two, B and D, at the intermediate points which the other two thermometers occupied between the middle of the axis and its two extremities.

Erecting the perpendiculars A f, B g, C h, D i, and E k, on the points A, B, C, D and E; and taking the ordinate A f = 212, the temperature of boiling water; B g = 162, the temperature indicated by the thermometer B; C h = $132\frac{3}{4}$, the temperature indicated by the thermometer C; D i = $106\frac{1}{2}$, the temperature given by the thermometer D; and laſtly, E k = 32, the temperature of water mixed with pounded ice; a curve, P Q, paſſing through the points f, g, h, i, k, ought to be the *logarithmic*; that is, ſuppoſing the temperature of the ſurrounding air to be conſtantly at the temperature of melting ice during the experiment.

The curve has a point of contrary flexure.

But the experiment in queſtion was made when the temperature of the air was at 78° F. conſequently, reckoning from a certain point, taken in the length of the cylinder, where the temperature was at 78°, to the extremity E, the influence of the ſurrounding air, inſtead of *cooling* the ſurface of the cylinder, *heated* it; and it is evident, that the curve P Q muſt neceſſarily in this caſe have a point of inflexion.

It is likewiſe irregular.

In fact it appears on a ſimple inſpection of the figure, that the curve P Q has a point of inflexion; but we ſee likewiſe, that this curve is not regular. That branch which is concave toward the axis of the cylinder is not ſimilar to the adjoining portion of the curve, of equal length, which is convex toward that axis; as it ought to be according to our theory; and even the part of the curve which is convex toward the axis A E, differs ſenſibly from the logarithmic, particularly toward its extremity P.

The deviation is too much to be owing to the defects of thermometers.

It ought neceſſarily to differ from this curve, *as far as the deviſions of our thermometers are defective;* but the deviation be-

tween the ordinates A *f* and B *g*, indicated by the results of the experiment in question, appears to me much too considerable to be ascribed to the imperfection of our thermometers.

It differs greatly from the logarithmic.

To see how far the curve P Q differs from the logarithmic, we have only to draw a logarithmic curve R S through the points *g* and *i*, and we shall find, that the ordinates corresponding to the points

	A,	B,	C,	D,	E.
Instead of being	212°	162°	132°$\frac{3}{4}$	106$\frac{1}{2}$	32°
Will be -	199.55	162	131	106$\frac{1}{2}$	86.35
Difference -	−2·45	0	−1$\frac{3}{4}$	0	+54·35

Ascribed to water being a bad conductor of heat,

The very great difference that exists between the temperature of cold water, and that indicated by the results of the experiment for the extremity of the cylinder which was in contact with this water, led me to suspect, that it was owing to the quality possessed by water in common with other fluids, which renders it *a very bad conductor of heat.*

—and the currents in the cold water being inconsiderable.

If it be true, as I believe I have elsewhere proved, that there is no sensible communication of heat between the adjacent particles of a fluid, from one to another; and that heat is propagated through fluids only in consequence of a motion of their particles, resulting from a change in their specific gravity, occasioned by their being heated or cooled: as the specific gravity of water is very little altered by an inconsiderable change of temperature when this fluid is near the freezing point, it might have been foreseen, that a solid body a little heated, and plunged into cold water, would be very slowly cooled.

Experiment to prove this.

The result of the following experiment, which I made with a view to elucidate this point, will put the fact out of all doubt.

When the cold water was briskly stirred the thermometers were all greatly depressed.

The three thermometers being stationary, one, B, at 162°, the second, C, at 132$\frac{3}{4}$°, and the third, D, at 106$\frac{1}{2}$°, the water in contact with one of the extremities of the cylinder being still boiling, while the water mixed with pounded ice, which was in contact with the other extremity, was constantly at the temperature of melting ice, I began to stir this mixture of ice and water pretty briskly with a little stick, and I continued to stir it uninterruptedly, and with the same velocity, for two and twenty minutes.

I had scarcely begun this operation, when I had a proof, that

that my conjectures were well founded. The mercury in the three thermometers immediately began to deſcend, and did not ſtop till it had fallen very conſiderably.

The thermometer B fell from 162° to 152°; C from $132\frac{3}{4}$° to $111\frac{3}{4}$°; and D from $106\frac{1}{2}$° to $78\frac{1}{2}$°. Quantities of depreſſion.

On comparing theſe numbers we find, that, in conſequence of the agitation of the cold water for two and twenty minutes, the thermometer B fell 10° of Fahrenheit's ſcale, the thermometer C 21°, and the thermometer D 28°.

As ſoon as I had ceaſed to ſtir the cold water, the three thermometers began to riſe, and at the end of a quarter of an hour they had all reached the points from which they ſet out at the beginning of this operation.

To facilitate the compariſon of the reſults of theſe two experiments, one made with cold water at reſt, the other with the ſame water in a ſtate of conſtant agitation, I have repreſented them in *Fig.* 4. Diagram to repreſent theſe effects.

In the firſt place we ſhall learn ſeveral very intereſting facts by ſimple inſpection of this figure; we ſhall ſee, 1ſt. that the progreſs of refrigeration, or, to ſpeak more properly, *the decreaſe of temperature*, was every where much more rapid, when the cold water in contact with the extremity of the cylinder E was agitated when it was at reſt. Obſervations upon this experiment.

2dly. That the extremity of the cylinder in contact with this water was conſtantly near 30° colder in the firſt caſe than in the ſecond.

3dly. We ſhall ſee, that the progreſs of refrigeration was every where, and in both the experiments, ſuch nearly as our theory points out.

The decreaſe of temperature toward the middle of the cylinder was ſo regular, that it is more than probable the apparent irregularities toward the two extremities were occaſioned ſolely by the difficulty which a body of water finds in communicating its mean temperature to a ſolid, with which it is in contact.

The boiling water being in continual motion owing to its ebullition, it had a great advantage over the cold water, which was at reſt, in communicating its temperature to the extremity of the cylinder it touched; but I have found, notwithſtanding this, that by agitating the boiling water ſtrongly with a quill, and particularly when with the quill I made a rapid Agitation increaſed the effect of the boiling water likewiſe.

friction againſt the end of the cylinder immerſed in the boiling water, I occaſioned all the thermometers to riſe ſeveral degrees.

The difference between the experiment and the theory confirm its truth;

It may perhaps be imagined, at firſt ſight of the reſults of the experiment, that, as the three thermometers, which occupied the parts about the middle of the axis of the cylinder, did not indicate a decreaſe perfectly agreeing with the theory, the theory itſelf cannot be true: but a moment's reflection will ſhow, that this inference would be too haſty, and that the difference between the theory and the reſults of our experiments, far from proving any thing adverſe to the theory, ſerve on the contrary to render it more probable.

becauſe the ſcales of our thermometers are defective.

The reſults of ſuch experiments can never agree with the theory, except the diviſions of our thermometers be perfectly accurate: but it is well known to every one, who has any knowledge of natural philoſophy, that the diviſions of our thermometers are defective.

To improve this inſtrument is an object of importance.

One of the objects I had in view in the experiments, of which I have juſt given an account to the claſs, and in ſeveral others, which I intend to make without delay, is to improve the diviſion of the ſcale of the thermometer, in order to render this valuable inſtrument of greater utility in the delicate inveſtigations of natural philoſophy.

The air thermometer deſerves to be attended to.

It appears certain, that the increaſe of the elaſticity of air by heat is much more nearly proportionate to the increaſe of temperature, than the dilatation of mercury or any known fluid; conſequently it is the air thermometer we ought to endeavour to improve, and which muſt ultimately afford us the moſt accurate meaſure of heat, that it is poſſible for us to procure.

SECT. IV. *The Heat produced in a Body by a given Quantity of ſolar Light is the ſame whether the Rays be denſer or rarer, convergent, parallel, or divergent.*

Whether the quantity of heat generated by the ſolar rays be proportional to the light abſorbed.

In all caſes where the rays of the ſun ſtrike on the ſurface of an opake body without being reflected, heat is generated, and the temperature of the body is increaſed: but is the *quantity* of heat thus excited always in proportion to the quantity of light that has diſappeared? This is a very intereſting queſtion, and has not hitherto found a deciſive ſolution.

When

When we conſider the prodigious intenſity of the heat excited in the focus of a burning mirror or a lens, we are tempted to believe, that the concentration and condenſation of the ſolar rays increaſe their power of exciting heat; but, if we examine the matter more cloſely, we are obliged to confeſs, that ſuch an augmentation would be inexplicable. It would be equally ſo on both the hypotheſes, which natural philoſophers have formed of the nature of light: for, as it has been proved both by calculation and experiment, that two undulations in an elaſtic fluid may approach and even croſs each other, without deranging either their reſpective directions or velocities, if light be analogous to ſound, we do not ſee how the concentration or condenſation of theſe undulations can increaſe their force of impulſe: and if light be a real emanation, as its velocity is not altered, either by the change of direction it undergoes in paſſing through a lens, or by its reflection from the ſurface of a poliſhed body, it ſeems to me, that the power of each of theſe particles to excite or impart heat, muſt neceſſarily be the ſame after refraction or reflection as before; and conſequently, that the heat communicated or excited muſt be, in in all caſes, as the quantity of light abſorbed.

It does not follow from reaſoning that the *abſolute* power of the rays to produce heat can be increaſed by condenſing them.

I have juſt made ſome experiments, which appear to me to eſtabliſh this fact beyond queſtion.

Experimental inveſtigation.

Having procured from the optician Lerebours two lenſes perfectly equal, and of the ſame kind of glaſs, four inches in diameter, and of eleven and a half focus, I expoſed them at the ſame time to the ſun, ſide by ſide, about noon, when the ſky was very clear; and by means of two thermometers, or reſervoirs of heat, of a peculiar conſtruction, I determined the relative quantities of heat, that were excited in given times by the ſolar rays at different diſtances from the foci of the lenſes.

Two convex lenſes perfectly ſimilar were uſed,

The two reſervoirs of heat are a ſort of flat boxes of braſs filled with water. Each of theſe reſervoirs is three inches ten lines and a half in diameter, and ſix lines thick, well poliſhed externally on all ſides except one of its two flat faces, which was blackened by the ſmoke of a candle. On this face the ſolar rays were received in the experiments.

to throw the ſun's light upon flat tin boxes containing water and blackened on their ſurfaces.

Each of theſe reſervoirs of heat weighs when empty 6850 grains, *poids de marc*, (near a pound troy), and contains 1210 grains of water (about 2 oz. 2 dwts.

Taking

Taking the capacity of braſs for heat to be to that of water as 0,11 to 1, it appears, that the capacity of the metallic box, weighing 6850 grains, is equal to the capacity of 622 grains of water; and adding this quantity of water to that contained in the box, we ſhall have the capacity of the reſervoir prepared for the experiments equal to that of 1932 grains of water.

The temperature of the water in each was ſhewn by a thermometer.

Each reſervoir is kept in its place by a cylinder of dry wood, one of the extremities of the cylinder being fixed in a ſocket in the center of the interior face of the reſervoir; and each reſervoir has a little neck, through which it is filled with water, and which after receives the bulb of a cylindrical thermometer, that reaches completely acroſs the inſide of the box in the direction of its diameter.

The two reſervoirs of heat, with their two lenſes, are firmly fixed in an open frame, which being moveable in all directions by means of a pivot and a hinge, the apparatus is eaſily directed toward the ſun, and made to follow its motion regularly, ſo as to keep the ſolar ſpectra conſtantly in the centers of the blackened faces of the reſervoirs.

Light admitted through equal apertures.

In order that the quantities of light paſſing through the two lenſes ſhould be perfectly equal, a circular plate of well poliſhed braſs, in the centre of which is a circular hole three inches and a half in diameter, is placed immediately before each of the lenſes.

When the reſervoirs of heat are placed at different diſtances from the focuſes of their reſpective lenſes, the diameters of the ſolar ſpectra, which are formed on the blackened faces of the reſervoirs, are neceſſarily different; and as the quantities of light are equal, its denſity at the ſurface of each reſervoir is inverſely as the ſquare of the diameter of the ſpectrum formed on that ſurface.

Experiment I.

Experiment. With equal apertures the ſolar ſpots from the lenſes were of 6 and of 24 lines diam.

In this experiment the reſervoir A was placed ſo near the focus of the lens, between the lens and the focus, that the diameter of the ſolar ſpectrum falling on it was only $\frac{1}{2}$ an inch, or 6 lines, while the reſervoir B was advanced ſo far before the focus, that the ſpectrum was two inches in diameter, or 24 lines.

As

As the quantities of light falling on both were equal, the denſity of the light at the ſurface of the reſervoir A was to the denſity of that at the ſurface of the reſervoir B, as the ſquare of 24 to the ſquare of 6, or as 16 to 1.

The denſities of theſe equal quantities of light were therefore as 16 to 1;

I imagined, that, if the quantity of heat, which a given quantity of light is capable of exciting, depended any way on its denſity, as the denſities were ſo different in this experiment, I could not fail to diſcover the fact by the difference of time, which it would require to raiſe the two thermometers the ſame number of degrees.

Having continued the experiment more than an hour, on a very fine day, when the ſun was near the meridian and ſhone extremely bright, I did not find, that one of the reſervoirs was heated perceptibly quicker than the other.

but both the veſſels were heated in equal times.

Experiment II.

I placed the reſervoir of heat A ſtill nearer the focus of the lens, in a ſituation where the ſolar ſpectrum was only $4\frac{3}{4}$ lines in diameter, and where blackened paper caught fire in two or three ſeconds; and I removed the reſervoir B ſtill farther from the focus, advancing it forward till the diameter of the ſpectrum was two inches three lines.

Experiment wherein the diameters of the ſpots were as $4\frac{3}{4}$ to 27.

The denſities of the light at the ſurfaces of the reſervoirs in this experiment were as 32 to 1.

The denſities of the light were as 32 to 1.

The temperature of the reſervoirs, as well as that of the atmoſphere, at the beginning of the experiment, was 54° F. = 9° $\frac{7}{9}$ R.

The reſervoir A, after having been expoſed to the action of very intenſe light near the focus of the lens for twenty-four minutes forty ſeconds, was raiſed to the temperature of 80° F. = 21° $\frac{1}{3}$ R.

The denſeſt light afforded rather leſs heat.

The reſervoir B, which was much farther from the focus of its lens, was raiſed to the ſame temperature, 80° F. a little more quickly, or in twenty-three minutes forty ſeconds.

To raiſe the temperature of the reſervoir A to 100° F. = 30° $\frac{2}{9}$ R. it was neceſſary to continue the experiment for one hour fifteen minutes ten ſeconds, reckoning from the commencement of it; but the reſervoir B reached the ſame temperature in one hour twelve minutes ten ſeconds.

The progreſs of this experiment from the beginning to the end is exhibited in the following table:

Increaſes

The general results tabulated.

Increaſes of Temperature.	Time taken By A.	Time taken By B.
From 54° to 80° F.	24′ 40″	23′ 40″
80 85	7 45	7 30
85 90	9 55	9 0
90 95	13 30	13 0
95 100	19 20	19 0
54 100	75 10	72 10

Time of the experiment.

This experiment was begun at 7 minutes 30 ſeconds after 11, and finiſhed at 22 minutes 40 ſeconds after 12, the ſky being perfectly clear during the time.

Hence light does not give more heat abſolutely by being condenſed.

On comparing all the reſults of this experiment, we ſee, that the reſervoir A, which was placed very near the focus, was more ſlowly heated than the reſervoir B, which was at a conſiderable diſtance from it *. The differences of time however taken to heat them an equal number of degrees were very trifling, and I think may be eaſily explained, without ſuppoſing the condenſation of light to increaſe (*qu.* diminiſh?) its faculty of exciting heat.

The rays were convergent in the preceding experiments.

In both the preceding experiments the ſolar rays ſtriking on the reſervoirs of heat were *convergent*, and they were even equally ſo on both ſides. To determine whether *parallel* rays have the ſame power of exciting heat as convergent rays, I made the following experiment.

Experiment III.

When one veſſel was expoſed to the parallel rays of the ſun without interception,

Having removed the lens from before the reſervoir B, I ſuffered the direct rays of the ſun to fall on the blackened face of the reſervoir, through the circular hole three inches and half in diameter in the round braſs plate, which had been conſtantly placed before that lens in the preceding experiments.

The reſervoir A was placed behind its lens as in the former experiments, and at the place where the ſolar ſpectrum had ſix lines diameter.

* Did not the elevated temperature of the ſmaller ſurface ſuſtain its power of abſorbing heat, conformably to the known laws of heated bodies?—N.

Having

Having expoſed this apparatus to the ſun, I found, that the reſervoir B, on which the direct rays fell, was heated ſenſibly quicker than the reſervoir A, which was expoſed to the action of the concentrated rays near the focus of the lens.

it was heated more quickly than the other by a ſpectrum of 6 lines or one-ſeventh part diam.

The temperature of the apparatus and of the atmoſphere at the beginning of the experiment being 53° F. = 9° $\frac{4}{9}$ R. the reſervoir A required twenty-three minutes thirty ſeconds to raiſe it to the temperature of 80° F. = 21° $\frac{2}{9}$ R; but the reſervoir B, which was expoſed to the direct rays of the ſun, acquired the ſame temperature in eighteen minutes thirty ſeconds.

To reach the temperature of 100° F. = 30° $\frac{2}{9}$ R. took the reſervoir A one hour and three minutes, but the reſervoir B forty-ſeven minutes fifteen ſeconds only.

The following table will ſhow the progreſs of this experiment from the beginning to the end.

General reſults.

Increaſes of Temperature.	Time taken By A.	Time taken By B.
From 53° to 65° F.	8′ 26″	7′ 0″
65 70	4 10	3 15
70 75	5 10	3 45
75 80	5 40	4 30
80 85	7 0	4 45
85 90	7 30	5 45
90 95	10 30	8 0
95 100	13 10	10 15
100 105	20 0	14 45
53 105	81 36	62 30

This difference aſcribed to light being loſt in paſſing through the lens.

As a conſiderable part of the light that fell on the lens before the reſervoir A, was loſt in paſſing through it, it is evident, that the quantity received by this reſervoir was leſs than that received by the reſervoir B, which was expoſed to the direct rays of the ſun; and we have ſeen, that the latter was heated more rapidly than the former.

This experiment is not deciſive: but

As we know not exactly how much light was loſt in paſſing through the lens, we cannot determine from the reſults of this experiment, whether convergent rays be more or leſs efficacious in exciting heat than parallel rays; but the difference in the times

times of heating was not greater, as it appears to me, than we might have expected to find it, ſuppoſing it to be occaſioned ſolely by the difference between the quantities of light acting on the reſervoirs.

The reſult of the following experiment will eſtabliſh this point beyond doubt.

Experiment IV.

Exp. 4 was made with equal apertures and ſpectra; but the one being *within* the focus was formed by convergent rays, and the other *without* by divergent rays.

Having replaced the lens belonging to the reſervoir B, I adjuſted this reſervoir to ſuch a diſtance between the lens and its focus, that the ſolar ſpectrum was one inch in diameter; and I placed the reſervoir A at the ſame diſtance beyonds its focus.

As the quantities of light directed toward both were equal; and the diameters of the ſpectra, conſequently the denſities of the light that formed them, were alſo equal; there could be no difference between the reſults of the experiments with the two reſervoirs, except what was occaſioned by the difference in the *direction* of the rays that formed the ſpectra. On one hand theſe rays were *convergent*, and on the other *divergent*; and I had inferred, that if parallel rays were in reality leſs efficacious in exciting heat than convergent rays, as ſome philoſophers have ſuppoſed, *divergent* rays muſt be ſtill leſs efficacious than parallel rays, and conſequently much leſs than convergent rays.

No ſenſible difference occurred.

Having made the experiment with all poſſible care, I found no ſenſible difference between the quantities of heat excited in a given time by divergent and convergent rays.

The following are the particulars of the progreſs and reſults of this experiment:

General reſults of this laſt experiment.

Increaſes of Heat.	Time taken	
	By A, with divergent Rays.	By B, with convergent Rays.
From 60° to 65° F.	4′ 50″	4′ 50″
65 70	4 55	5 0
70 75	5 27	5 25
75 80	6 13	6 15
60 80	21 25	21 30

From

From the results of all the experiments, of which I have just given an account to the class, we may conclude, that the quantity of heat excited or communicated by the solar rays is always, and under all circumstances, as the quantity of light that disappears.

Conclusion. The quantity of heat is always as the light absorbed.

III.

Observations on blasting Rocks; with an Account of an Improvement, whereby the Danger of accidental Explosion is in a great Measure obviated. By Mr. William Close. *From the Author.*

To Mr. NICHOLSON.

SIR, *Dalton, Oct.* 14, 1805.

THE method of confining the force of gunpowder by a column of sand in blasting rocks, has been several years used in this part of Furness: At one time it was a very favourite practice; but at present, from the prejudices or indifference of workmen, or on account of the little danger attendant on working lime-stone in the common manner, it is less in repute.

Practice of blasting with sand in Furness,

About two years ago, supposing this method not to be generally known, I drew up a short account of it, and should have sent it to the Philosophical Journal, had it not been connected with other miscellaneous matter, which I had given to Mr. G. Ashburner, the printer and proprietor of a new edition of West's Antiquities of Furness, in which work the process is described and recommended *.

noticed by the author elsewhere.

Though

* The passage alluded to is as follows, p. 393. "In breaking up the loose rocks upon Baycliff Haggs, after the enclosure of that common, a method of employing sea-sand, for the purpose of confining the force of gunpowder in blasting, was used, which does not appear to be generally known, though it was undoubtedly in use in other parts before it was adopted in Furness. The method is briefly this: After the excavation is made in the usual manner with a borer, the charge of powder is poured in; and a priming-straw of a proper length, filled with powder, is placed in the hole, having one of its sides near the lower end so cut or thinned, that the

Improvement by Mr. Fiſher announced.

Though this method is undoubtedly worthy of much attention, and may often be employed with advantage; yet, when a ſtrong charge is required, the common mode of ſtemming muſt be frequently adopted: And as the danger in blaſting ſome kinds of rocks in this manner is very conſiderable, I am happy to notice an eaſy method of obviating one principal cauſe of accidental exploſion, which was communicated to me in converſation, a few days ago, by Mr. Thomas Fiſher, a reſpectable ſlate merchant in this town, who aſſures me it is infallible.

Cauſes of accidental exploſion. The principal is from the friction of the iron pricker in drawing.

The principal danger attendant on blaſting, does not conſiſt in ſtemming upon the charge of powder, but in the ſubſequent operation of drawing the iron rod, called the pricker, which makes the channel for the priming-ſtraw. For although the colliſion of the firſt fragments of ſtemming ſometimes produces an exploſion, yet this may be prevented by previouſly ramming a thick cap of paper, &c. upon the powder; by beating lightly upon the firſt pieces of ſtone that are thrown into the hole; or by uſing thoſe materials for ſtemming which are leaſt liable to give fire, ſuch as rotten ſtone, pieces of broken pots, or burnt clay. The pricker being hard preſſed againſt the rock, and in cloſe contact with the ſtemming, cannot be drawn out by hand, but muſt be ſtruck out by the hammer, a ſtrong piece of iron called a jumper being firſt placed in an eye or loop in the higheſt part of the rod, to receive the blows which are given in a proper direction to bring it out of its place. Now it frequently happens, that the friction of the loweſt part of the pricker againſt the rock fires the powder at the firſt or ſecond blow. When the exploſion happens at the commencement of ſtemming, the workman generally ſuſtains only a partial injury; but when in this part of the operation, when the powder exerts its whole force, and

the charge may partially communicate with the ſmall aſcending column contained in the ſtraw. After this, the remainder of the excavation is filled, by pouring in dry ſea-ſand; and the exploſion is given, by firing the priming-ſtraw in any of the various ways which are in common uſe.

"This method has been found to be equally effectual as ſtemming with any of the common materials; and where it can be uſed is certainly preferable: it is ſafer, ſimpler, and more expeditious."

diſperſes

diſperſes pieces of the ſhattered rock in various directions, his life is in the utmoſt danger, and his ſituation is truly terrible to contemplate.

Mr. Fiſher's improvement is to obviate this danger; and conſiſts in the uſe of a copper rod, or pricker, for making the hole that receives the priming-ſtraw, inſtead of one of iron, which before was every where employed in this part of the kingdom.

Mr. F.'s expedient is a pricker made of copper, which is not liable to fire the powder.

In our converſation Mr. F. obſerved, that ſome years ago, three exploſions happened on drawing the pricker, in the courſe of a fortnight, at his quarry in Kirkby Ireleth, and that one man being killed and two wounded, ſeveral of the workmen were ſo intimidated, that they reſolved to abandon a place which they conſidered as deſtined to daily misfortunes. It therefore became highly requiſite, on ſeveral accounts, to attempt ſome innovation for the ſecurity and encouragement of the workmen.

In meditating on the cauſe of theſe accidents, it appeared moſt rational to attribute them to the iron pricker giving fire by its friction againſt the rock, which was a hard blue rag, or whinſtone: and from this view of the cauſe it was inferred, that ſafety would accrue from the uſe of prickers conſtructed of thoſe metals which are leaſt diſpoſed to give fire with ſtone. Mr. Fiſher, therefore, determined to make trial of copper, and having procured ſome implements of this kind, found them to anſwer the purpoſe completely. It is now upwards of three years ſince this improvement was adopted, and as no exploſion has happened at the end of ſtemming in that period, at an extenſive work where accidents were frequent before, Mr. F. conſiders the means as almoſt infallible; and is happy to think that many ſad misfortunes have been thereby prevented.

Ample experience has proved its utility.

There are eleven ſlate quarries in Kirkby Ireleth, at ſeveral of which copper rods are now uſed; but at others they are not. At one of theſe a fatal accident happened a few months ago, from an exploſion upon drawing a rod of iron.

Prickers, ſuch as uſed by Mr. Fiſher, are eaſily conſtructed: A piece of copper being forged to the proper length, ſhape, and thickneſs for the body of the tool, is rivetted to an iron head or loop ſimilar to that of the common pricker. Theſe implements, when carefully uſed, are nearly as durable as thoſe of iron.

Advantage of ſtrong charges in firm rock.

Sand has not hitherto been uſed in blaſting at the ſlate quarries in Kirkby Ireleth. The maſters do not think it would ſucceed well in their work. I have frequently ſeen Mr. Fiſher uſe it in limeſtone rock near this town: He ſays it anſwers the beſt in deep holes, but thinks that ſand is more liable to be blown out than ſtemming. He alſo conſiders it as the moſt advantageous method of working, in driving levels, and blaſting in firm rock, to uſe ſtrong charges of powder, that the ſtone may be ſufficiently broken by the exploſion to be removed without much aſſiſtance from the hammer, the pick, or the lever: For thus the expedition of the work amply compenſates for the ſmall addition which is requiſite to a common charge of powder.

I am, Sir,

Your's reſpectfully,

WILLIAM CLOSE.

IV.

Deſcription of a portable Steam-engine, invented by Mr. SAMUEL CLEGG*, *David Street, Mancheſter. Communicated by Mr.* DALTON, *Lecturer at the Royal Inſtitution, &c.*

Deſcription of a ſteam-engine.

THIS engine is worked by four copper valves in the uſual manner, but the mechaniſm for lifting them is very different from any hitherto made: there are no levers employed for opening the valves, and there is no hand gear. The ſteam and exhauſtion valves are on the ſame horizontal plane; thoſe which are vertical to each other are not like thoſe hitherto uſed, both expoſed to the ſteam or both to a vacuum; but by a ſimple contrivance in the conſtruction of the nozzles, the one is expoſed to the ſteam while the other has a communication with the condenſing veſſel. From what has been ſaid it may eaſily be perceived, if the two valves be connected together by a ſtraight rod, that when this rod is lifted, the preſſure is given to the piſton, and the machine is put into motion; and if the other two valves be connected in the ſame

* Late apprentice to Meſſrs. Boulton and Watts, of Birmingham.

manner

manner and lifted at an appointed time, the engine is kept in motion. The outſide appearance of theſe nozzles may be ſeen at *Fig.* 1, *c c*, (*Plate* IX.) The rods which come out of the bottom of the nozzles are kept tight by vertical ſtuffing-boxes, the whole of which is hid in the drawing by the frame. Deſcription of a ſteam-engine.

The next is a new contrivance for producing a rotative motion from a reciprocating one, which not only ſimplifies the machine very much, but exceeds the power of the common crank by nearly one-third, in conſequence of its acting always perpendicular to the radius of the wheel, which is done by a rack and wheel, as repreſented by *Fig.* 2 and 3; and as this plan of connection diſtributes the power uniformly, of courſe a much lighter fly-wheel is required, which diminiſhes friction, &c.

Explanation of the Plate.

Fig. 1. is a repreſentation of the engine: one of the corner columns A A, which ſupports the frame, ſerves likewiſe for an eduction-pipe and condenſing-veſſel: the air-pump E is joined to the condenſing veſſel by the pipe D; *e* is the piſton-rod, and though it works out at the bottom of the cylinder, it is as eaſily kept tight as if it worked out at the top; *b* is a ſimilar rod which keeps the rack perpendicular; *a a* are the two radius bars on which the braſſes are fixed that ſupport the ſhaft; by this contrivance the wheel C eaſily moves from one ſide to the other of the rack F.

Fig. 2. is a view of the rack on a larger ſcale, where C repreſents the wheel and D the ſhaft; E E, a ſliding-bar, on which is fixed the ſmall roller *o*, ſerving as a connecting link to keep the wheel C always in gear; for, when the wheel is in gear on the oppoſite ſide of the rack, the roller *o* is on the other ſide of the plate *a a*; but it will perhaps be more clearly underſtood by the plan, *Fig.* 3. where the letters repreſent the ſame movement as in the elevation, *Fig.* 2: This deſcription may be eaſily underſtood by thoſe who already poſſeſs a little knowledge of a ſteam-engine.

Mancheſter, Oct. 5, 1805.

Letter

V.

Letter from Mr. J. C. HORNBLOWER, *describing the framed Work by which the Roof of Clapham Church was raised to its original Situation, without disturbing the Interior of the Building, &c.*

To Mr. NICHOLSON.

DEAR SIR,

Framed truss by which the roof of Clapham church was raised, &c.

IT will be a pleasure to you I know to record the productions of genius or fancy in your valuable Work, and therefore I have no hesitation in presenting the inclosed for that purpose.

It is the invention of Mr. Watkin Bloore, one of the partners of Fothergal and Co. carpenters at Clapham, and was invented to raise the sunk roof of Clapham church; and its application to the purpose intended, exhibits at once the *means* and the *end* that was to be accomplished; as by it the roof was *raised* and *secured* in the same process, without incommoding the building with shoors and scaffolds, which must have occasioned considerable damage to the furniture of the church.

The shaded part of the drawing, *Plate* X. shews the truss, and the lines behind it the construction of the roof. The middle piece in the truss marked A, is joggled into the king-post of the roof, and the two screws put into action raise it up, and with it the whole of the middle or sunk part of the roof, all which is easily comprehended by the drawing.

The drawing, *Fig.* 3, shews an improved mode of constructing the truss, by the riders A A A A being framed over the principals B B, by which the raising screws are more firmly supported in elevating the queen-posts C C in the roof.

This must be a valuable experiment in the art of carpentry, which, considering how little science of it falls to the lot of its possessors, cannot be too much regarded.

I am, Dear Sir,

Your very obedient servant,

J. C. HORNBLOWER.

Remarks

VI.

Experiments on draining Land, by JOHN CHRISTIAN CURWEN, *Eſq. M. P. of Workington-Hall, in Cumberland, with an Engraving* *.

DEAR SIR,

MUCH having been ſaid, in the public Papers, relative to draining, on the improved method of Mr. Elkington, I beg leave to offer you ſome obſervations reſpecting it, which have fallen under my notice, and which tend to prove it can be applied, with ſucceſs only, in ſuch parts of the kingdom, as have few, if any, interruption of the ſtrata. In order to make myſelf intelligible, it may not be improper to explain what is meant by interruptions of the ſtrata, or dykes and fiſſures, as they are denominated in mining countries. They are produced by the fracture or diſunion of the ſtrata, and conſiſt moſt commonly of the broken fragments of each ſuperior ſtrata; and towards the ſurface are of ſand, gravel, and ſtones, which ſeldom or never fail of affording conſiderable quantities of water. Theſe dykes may be approached within a few feet, and afford no water, as will be ſeen in two inſtances in the plan ſent you. No. 3 is a main drain, four feet deep, which paſſed within a few yards of A, an extreme wet place, and did not affect it. The perſon employed, ſuppoſed the water to be below him, and brought in a lower level No. 1, which likewiſe failed. No. 2 was then made ſtill lower, but with no better ſucceſs than No. 3, though with more advantage of level. As ſoon as it croſſed the dyke, I C, but before the level was brought up, not being deeper than the main drain, it got a conſiderable feeder. This proved that an interruption in the ſtrata prevented the water flowing into a drain, which was of a depth otherwiſe to have drawn it. Another example occurs in the ſame field, at letter B; which is a ſunk fence, four feet below the ſurface of the adjoining field, which was extremely wet within a few yards of the ſunk

Mr. Elkington's method of draining is applicable only where the ſtrata are little interrupted.

Dykes and fiſſures.

Theſe interruptions prevent the draining off of the water.

* From the twenty-ſecond volume of the Tranſactions of the Society of Arts; who awarded the gold medal to the author. The plan he refers to is at their houſe.

Experiments of drainage.

fence. A lower level was ſuppoſed neceſſary to drain this water, and it was obtained at the dotted line. No water of any conſequence was got, till it was within a few yards of the ſunk fence, when a prodigious feeder was cut, and the head of the drain was not ſo deep at the time as the ſunk fence. Many inſtances to the ſame effect might be produced. In ſinking ſhafts in places much troubled with water, it is endeavoured, if circumſtances will permit, to get near a dyke, which ſerves as a barrier to the water; and if, in ſinking, the dyke be not croſſed, the water is kept clear off; but if otherwiſe, the water would be got at any depth, though not in ſuch quantities as when near the ſurface. The ſpot of ground, to which I have alluded, has above a dozen dykes, which may be traced from the out-burſts of water. They run in a direction of ſouth to north-weſt. I have made my drains eaſt and weſt. In one or two places, I was obliged to run a drain ſouth. This proceeds from an arm running from the dyke: but this ſeldom extends to any diſtance, and they gradually decreaſe till they end; and they rather make an interruption than a breakage of the ſtrata, as the ſtrata is the ſame on each ſide of it. In ſuch a country, Mr. Elkington could draw no more water than what lay in the uninterrupted ſtrata between any two of theſe dykes. The method of making the drain is explained by the engraving. I had twenty years ago drained this ground with ſtone drains, from 20 inches to two feet; but their direction having been moſtly from north to ſouth, and not ſufficiently deep, I had got little more than the day water. The feeder which I have now got, might be made applicable to many purpoſes. The drains are from two feet to nearly five feet deep. I have made 6000 yards in the laſt twelve months; the cutting from 14d. to 18d. per rod, filling 8d. ten and a half ſingle cart-loads of ſtones, at 9d. each, making the coſt 10s. per rod. The expence appears great; but fewer drains are required, and the work is effectually done. By reference to the plan, it will be ſeen that the direction of the drains not being able to draw the upper water, I was obliged to extend them. I would adviſe beginning at the higheſt level; for frequently that clears the whole, unleſs ſome dykes intervene in a contrary direction. I believe that all ſprings and out-burſts of water proceed from dykes. The extent of theſe is various. Some may be traced for many miles, and their effects

The drains muſt be made to croſs the dykes.

Deſcription of the drains by reference to the drawing.

Springs of water proceed from dykes.

ſeen

ſeen from the water that appears on the ſurface. Their origin is ſcarcely perceptible, and they thicken to many yards as they are approached. The ſtrata on both ſides have a more rapid riſe or dip, and are of a cloſer and harder texture. If theſe obſervations appear to you worthy of attention, you may make what uſe you think proper of them. I by no means wiſh to detract from Mr. Elkington's merit; but it is not generally applicable; and in counties where the ſtrata are much broken, Mr. Elkington's plan will be found to fail.

I am, Dear Sir,

Your obedient ſervant,

J. C. CURWEN.

Feb. 3, 1804.

Mr. Charles Taylor.

P. S. The higheſt drain is 120 feet above the level of No. 3.

A certificate from Mr. William Hoodleſs, farming agent, accompanied this letter, ſtating that upwards of ſix thouſand yards of drains had been cut, and completely filled, on the farm of John Chriſtian Curwen, Eſq.; that the firſt drains made according to that plan were done three years ago; and that they ſtand completely, and anſwer an admirable purpoſe.

Reference to Fig. 5, Plate XI. *of the Manner in which Mr.* CURWEN'S *Drains are made.*

The loweſt part of the drain below E E is twelve inches wide.

E E 4 4 are the two ſide-ſtones of the drain, nearly four inches thick and nine inches high.

F 9 is the aperture for the water, nine inches high.

D, the flag or thin ſtone over the aperture, and which covers the ſide-ſtones of the drain.

C C, the body of the drain, filled with looſe ſtones till within nine inches of the ſurface.

B 9, the top of the drain, twenty-two inches wide and nine inches deep, filled with graſs ſod and ſoil.

VII.

Remarks on a Letter of Mr. DALTON, *concerning the Maximum Denſity of Water; with an Account of two Experiments of Dr.* HOPE, *tending to ſhew that it takes place at a Temperature above the freezing Point. In a Letter from* T. I. B.

To Mr. NICHOLSON.

SIR,

Reference to Mr. Dalton's letter.

IN No. 45 of your Journal, page 28, Mr. Dalton has publiſhed ſome remarks upon Count Rumford's experiments, relating to the maximum denſity of water, where he explains the riſing of the thermometer in the cup, by obſerving that it acquired heat by the proper conducting power of water. This, I ſhould think, is by no means probable; for the conducting power of water is not ſufficient to produce ſuch a rapid effect.

The circumſtances of the two thermometers by the ſide o the ball and cup, in the Count's two firſt experiments, I think are perfectly conſiſtent with his principle: the cup, very probably, did overflow, which might have been aſcertained by a thermometer placed below.

In the concluſion of this letter Mr. Dalton expreſſes a wiſh that Count R. or ſome one in poſſeſſion of a ſimilar apparatus, would repeat the Count's firſt experiment, with this difference, that the maſs of water ſhould be at 40° and the ball at 32°, in which caſe, he ſays, the thermometer would not be at all affected on the Count's principle; neither would it be affected (on Mr. D.'s principle) if the tenacity of the water counteracts the force of deſcent: and what concluſion could be drawn from ſuch a variation of the experiment?

Experiments of Dr. Hope.

The following experiment was made by Dr. Hope, profeſſor of chemiſtry in Edinburgh, to aſcertain the point at which water has the greateſt denſity,—and it appears to me to be perfectly deciſive.

He filled a jar with ice-cold water, and expoſed it to the air of a room at 52°: he ſuſpended in it two very delicate thermometers, one at half an inch from the bottom, and the other at the ſame diſtance from the ſurface of the water: the thermometer neareſt the bottom was firſt affected, and con-

tinued to rise till it reached the temperature of 40°, when it became stationary: the thermometer at the surface rose more slowly, but did not stop till it acquired the temperature of the room (52°).

Dr. Hope, to render this point still clearer, exposed water at 52° to the air of a room 32°; the result corresponded perfectly with the former experiment.

If you see no objection to the publication of this letter, by inserting it in your Journal, you will oblige *,

Sir, your's,

T. I. B.

Edinburgh, Oct. 10, 1805.

VIII.

Observations and Conjectures relative to the supposed Welch Indians in the western Parts of North America. Republished from the "Kentucky Palladium," with additional Remarks and Conjectures, by the Editor of the Philadelphia Medical and Physical Journal †.

SIR,

Traditional account of inhabitants of America supposed to have originated from Wales.

NO circumstance relating to the history of the Western Country, probably has excited, at different times, more general attention and anxious curiosity than the opinion, that a nation of white men, speaking the Welch language, reside high up on the Missouri. By some the idea is treated as nothing but the suggestion of bold imposture and easy credulity; whilst others regard it as a fact fully authenticated by Indian testimony and the report of various travellers worthy of credit.

The fact is accounted for, they say, by recurring to a passage in the history of Great Britain, which relates, that several years after the discovery of America by Christopher Columbus, a certain Welch prince embarked from his native country with a large party of emigrants; that after some time, a vessel

* A fuller account of the late experiments of Dr. Hope will be inserted when the Edinburgh Transactions appear.

† Extracted from that Work, Vol. II. Part I.

Traditional account of inhabitants of America ſuppoſed to have originated from Wales.

or two came back with the account that they had diſcovered a country far to the weſtward, and that they ſet ſail again with a freſh reinforcement, and never returned again any more.

The country which theſe adventurers diſcovered, it has been ſuppoſed, was the continent of North America; and it has been conjectured that they landed on the continent, ſomewhere in the Gulf of Mexico, and from thence proceeded northward, till they got out of the reach of the hoſtile natives, and ſeated themſelves in the upper country of Miſſouri.

Many accounts accordingly have been publiſhed, within the laſt thirty years, of perſons who, in conſequence either by accident or the ardour of curioſity, have made themſelves acquainted with a nation of men on the Miſſouri, poſſeſſing the complexion of Europeans and the language of Welchmen.

Could the fact be well eſtabliſhed, it would afford perhaps the moſt ſatisfactory ſolution of the difficulty occaſioned by a view of the various ancient fortifications with which the Ohio country abounds, of any that has ever been offered. Thoſe fortifications were evidently never made by the Indians. The Indian art of war preſents nothing of the kind. The probability too is that the perſons who conſtructed them were, *at that time*, acquainted with the uſe of iron: The ſituation of theſe fortifications, which are uniformly in the moſt fertile land of the country, indicates, that thoſe who made them were an agricultural people; and the remarkable care and ſkill with which they were executed, affords traits of the genius of a people, who relied more on their military ſkill than on their numbers. The growth of the trees upon them is very compatible with the idea that it is not more than three hundred years ago that they were abandoned.

Theſe hints however are thrown out rather to excite enquiry, than by way of advancing any decided opinion on the ſubject. Having never met with any of the perſons who had ſeen theſe white Americans, nor even received their teſtimony near the ſource, I have always entertained conſiderable doubts about the fact. Laſt evening, however, Mr. John Childs, of Jeſſamine County, a gentleman with whom I have been long acquainted, and who is well known to be a man of veracity, communicated a relation to me, which at all events appears to merit ſerious attention.

After

After he had related it in converſation, I requeſted him to repeat it, and committed it to writing. It has certainly ſome internal marks of authenticity. The country which is deſcribed was altogether unknown in Virginia when the relation was given, and probably very little known to the Shawnees Indians. Yet the account of it agrees very remarkably with later diſcoveries. On the other hand, the ſtory of the large animal, though by no means incredible, has ſomething of the air of fable, and it does not ſatisfactorily appear how the long period which the party were abſent was ſpent; though Indians are, however, ſo much accuſtomed to loiter away their time, that many weeks, and even months, may probably have been ſpent in indolent repoſe.

Traditional account of inhabitants of America ſuppoſed to have originated from Wales.

Without detaining you any more with preliminary remarks, I will proceed to the narration, as I received it from Mr. Childs.

Maurice Griffith, a native of Wales, which country he left when he was about ſixteen years of age, was taken a priſoner by a party of Shawnees Indians, about forty years ago, near Voſſes Fort, on the head of Roanoke river in Virginia, and carried to the Shawnees nation. Having ſtaid there about two years and a half, he found that five young men of the tribe had a deſire of attempting to explore the ſources of the Miſſouri. He prevailed upon them to admit him as one of the party. They ſet out with ſix good rifles and with ſix pounds of powder a-piece, of which they were, of courſe, very careful.

On reaching the mouth of the Miſſouri, they were ſtruck with the extraordinary appearance occaſioned by the intermixture of the muddy waters of the Miſſouri and the clear tranſparent element of the Miſſiſſippi. They ſtaid two or three days amuſing themſelves with the view of this novel ſight: they then determined on the courſe which they ſhould purſue, which happened to be ſo nearly in the courſe of the river, that they frequently came within ſight of it as they proceeded on their journey.

After travelling about thirty days through pretty farming wood land, they came into fine open prairies, on which nothing grew but long luxuriant graſs. There was a ſucceſſion of theſe varying in ſize, ſome being eight or ten miles acroſs, but one of them ſo long that it occupied three days to travel through

Traditional account of inhabitants of America ſuppoſed to have originated from Wales.

through it. In paſſing through this large prairie, they were much diſtreſſed for water and proviſions, for they ſaw neither beaſt nor bird; and, though there was an abundance of ſalt ſprings, freſh water was very ſcarce. In one of theſe prairies the ſalt ſprings ran into ſmall ponds, in which, as the weather was hot, the water had ſunk and left the edges of the ponds ſo covered with ſalt, that they fully ſupplied themſelves with that article, and might eaſily have collected buſhels of it. As they were travelling through the prairies they had likewiſe the good fortune to kill an animal, which was nine or ten feet high, and a bulk proportioned to its heighth. They had ſeen two of the ſame ſpecies before, and they ſaw four of them afterwards. They were ſwift-footed, and they had neither tuſks nor horns. After having paſſed through the long prairie, they made it a rule never to enter on one which they could not ſee acroſs, till they had ſupplied themſelves with a ſufficiency of jerked veniſon to laſt ſeveral days.

After having travelled a conſiderable time through the prairies, they came to very extenſive lead mines, where they melted the ore, and furniſhed themſelves with what lead they wanted. They afterwards came to two copper mines, one of which was three miles through; and in ſeveral places they met with rocks of copper ore as large as houſes.

When about fifteen days journey from the ſecond coppermine, they came in ſight of white mountains, which, though it was in the heat of ſummer, appeared to them to be covered with ſnow. The ſight naturally excited conſiderable aſtoniſhment; but, on their approaching the mountains, they diſcovered that, inſtead of ſnow, they were covered with immenſe bodies of white ſand.

They had in the mean time paſſed through about ten nations of Indians, from whom they received very friendly treatment. It was the practice of the party to exerciſe the office of ſpokeſman in rotation; and when the language of any nation through which they paſſed was unknown to them, it was the duty of the ſpokeſman, a duty in which the others never interfered, to convey their meaning by appropriate ſigns.

The labour of travelling through the deep ſands of the mountains was exceſſive; but at length they relieved themſelves of this difficulty, by following the courſe of a ſhallow river, the bottom of which being level, they made their way to the top of the mountains with tolerable convenience.

After

After paſſing the mountains they entered a fine fertile tract of land, which having travelled through for ſeveral days, they accidentally met with three white men in the Indian dreſs. Griffith immediately underſtood their language, as it was pure Welch, though they occaſionally made uſe of a few words with which he was not acquainted. However, as it happened to be the turn of one of his Shawnees companions to act as ſpokeſman or interpreter, he preſerved a profound ſilence, and never gave them any intimation that he underſtood the language of their new companions.

Traditional account of inhabitants of America ſuppoſed to have originated from Wales.

After proceeding with them four or five days journey, they came to the village of theſe white men, where they found that the whole nation was of the ſame colour, having all the European complexion. The three men took them through their villages for about the ſpace of fifteen miles, when they came to the council-houſe, at which an aſſembly of the king and chief men of the nation was immediately held. The council laſted three days, and as the ſtrangers were not ſuppoſed to be acquainted with their language, they were ſuffered to be preſent at their deliberations.

The great queſtion before the council was, what conduct ſhould be obſerved towards the ſtrangers. From their fire-arms, their knives, and their tomahawks, it was concluded that they were a warlike people. It was conceived, that they were ſent to look out for a country for their nation; that if they were ſuffered to return, they might expect a body of powerful invaders; but that if theſe ſix men were put to death, nothing would be known of their country, and they would ſtill enjoy their poſſeſſions in ſecurity. It was finally determined that they ſhould be put to death.

Griffith then thought it was time for him to ſpeak. He addreſſed the council in the Welſh language. He informed them, that they had not been ſent by any nation; that they were actuated merely by private curioſity, they had no hoſtile intentions; that it was their wiſh to trace the Miſſouri to its ſource; and that they ſhould return to their country ſatisfied with the diſcoveries they had made, without any wiſh to diſturb the repoſe of their new acquaintances.

An inſtant aſtoniſhment glowed in the countenances, not only of the council, but of his Shawnees companions, who clearly ſaw that he was underſtood by the people of the country. Full

confidence

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confidence was at once given to his declarations, the king advanced and gave him his hand. They abandoned the deſign of putting him and his companions to death, and from that moment treated him with the utmoſt friendſhip. Griffith and the Shawnees continued eight months in the nation; but were deterred from proſecuting their reſearches up the Miſſouri by the advice of the people of the country, who informed them, that they had gone a twelve month's journey up the river, but found it as large there as it was in their own country.

As to the hiſtory of this people, he could learn nothing ſatisfactory. The only account they could give was, that their foreſathers had come up the river from a very diſtant country. They had no books, no records, no writings. They intermixed with no other people by marriage, there was not a dark-ſkinned man in the nation. Their numbers were very conſiderable. There was a continued range of ſettlements on the river, for fifty miles, and there were within this ſpace three large water-courſes which fell into the Miſſouri, on the banks of each of which they were likewiſe ſettled. He ſuppoſed that there muſt be fifty thouſand men in the nation capable of bearing arms. Their cloathing was ſkins well dreſſed. Their houſes were made of upright poſts and the barks of trees. The only implement they had to cut them with, were ſtone tomahawks; they had no iron. Their arms were bows and arrows. They had ſome ſilver which had been hammered with ſtones into coarſe ornaments, but it did not appear to be pure. They had neither horſes, cattle, ſheep, hogs, nor any domeſtic nor tame animals. They lived by hunting. He ſaid nothing about their religion.

Griffith and his companions had ſome large iron tomahawks with them. With theſe they cut down a tree and prepared a canoe to return home in: But their tomahawks were ſo great a curioſity, and the people of the courtry were ſo eager to handle them, that their canoe was completed with very little labour. When this work was accompliſhed, they propoſed to leave their new friends: Griffith, however, having promiſed to viſit them again.

They deſcended the river with conſiderable ſpeed, but amidſt frequent dangers, from the rapidity of the current particularly when paſſing through the white mountains. When they reached the

the Shawnees nation, they had been abſent about two years and a half. Griffith ſuppoſed that when they travelled they went at the rate of about fifteen miles per day.

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He ſtaid but a few months with the Indians after his return, as a favourable opportunity offered itſelf to him to reach his friends in Virginia. He came with a hunting party of Indians to the head-waters of Coal-river, which runs into New-river not far above the falls. There he left the Shawnees and eaſily reached the ſettlements on Roanoke.

Mr. Childs knew him before he was taken priſoner, and ſaw him a few days after his return, when he narrated to him the preceding circumſtances. Griffith was univerſally regarded as a ſteady honeſt man, and a man of ſtrict veracity. Mr. Childs has always placed the utmoſt confidence in his account of himſelf and his travels, and has no more doubt of the truth of his relation, than if he had ſeen the whole himſelf. Whether Griffith be ſtill alive or not he does not know.

Whether his ideas be correct or not, we ſhall probably have a better opportunity of judging on the return of Captains Lewis and Clark; who, though they may not penetrate as far as Griffith alledged that he had done, will probably learn enough of the country to enable us to determine whether the account given by Griffith be fiction or truth.

I am, Sir,

Your humble ſervant,

HARRY TOULMIN.

Frankford, Dec. 12, 1804.

Additional Obſervations and Conjectures by the Editor.

THE ſtory of a Welch colonization of America has excited much curioſity, both in Europe and the United States: By many it is believed, while by others it is thought unworthy of any attention. By reaſon of the preſent rapid progreſs of ſettlement in America, the time cannot be remote when the truth or falſity of this ſtory will be completely eſtabliſhed. In the meanwhile I do not heſitate to conjecture, *that no traces of the deſcendants of the Welch prince will ever be diſcovered in the weſtern parts of North America.*

It

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It may not be improper to notice the tale upon which ſo many perſons in Europe at leaſt reſt their hopes of proving, in the moſt ſatisfactory manner, that the Welch have contributed to the peopling of America.

David Powel, a Welch hiſtorian, informs us, that on the deceaſe of Owen Guyneth, king of North Wales, a diſpute aroſe among his ſons concerning the ſucceſſion to the crown; and that Madoc or Madog, one of the ſons, "weary of this contention, betook himſelf to ſea, in queſt of a more quiet ſettlement *." We are informed, that "he ſteered due weſt, leaving Ireland to the north, and arrived in an unknown country, which appeared to him ſo deſirable, that he returned to Wales, and carried hither ſeveral of his adherents and companions. After this neither Madog nor his companions were ever heard of more. The voyage of Madog is ſaid to have been performed about the year 1170.

I have not ſeen Powel's work, but I learn that this hiſtorian, who lived in the reign of Queen Elizabeth, and conſequently at a great diſtance of time from the event which he records, adduces no better authority in ſupport of the voyage than a quotation from a Welch poet, "which proves no more than that he (Madog) had diſtinguiſhed himſelf by ſea and land †." Some few Welch words, ſuch as *gwrando*, to hearken or liſten, &c. are very feebly or unfortunately adduced by Powel, as circumſtances favourable to the truth of the Welch emigration.

When we conſider "that the Welch were never a naval people; that the age in which Madog lived was peculiarly ignorant in navigation;" that the compaſs was then unknown; the ſtory of the voyages of the Welch prince muſt I think be conſidered as extremely improbable. I am of opinion with Mr. Pennant, that "the moſt which they could have attempted muſt have been a mere coaſting voyage."

But it may be ſaid, we muſt appeal to facts; and that independently of the verſes of the Welch poet, and the arguments of the Welch hiſtorian, it ſeems highly probable that a colony of white people who ſpeak the Welch language, does actually exiſt in the weſtern parts of North America.

* Dr. Robertſon.

† Pennant's Artic Zoology, Introduction, p. cclxiii. &c.

I cannot,

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I cannot, I muſt confeſs, adopt this opinion. I readily allow, that the relations publiſhed by Mr. Toulmin and many other perſons, both in Europe and in America, are extremely curious. But theſe relations are very inconſiſtent with one another, particularly in what relates to the actual ſtate of improvement of the ſuppoſed Welſhmen. By ſome we are told they are very far advanced in improvement; by others that their improvement is not at all greater than that of the Red-men or Indians of America. At one time, they are ſaid to be in poſſeſſion of manuſcripts (and even printed books) at another time nothing of this kind is found among them. It muſt be confeſſed that Maurice Griffith's relation is, in ſeveral reſpects, more plauſible than that of any preceding traveller; but it is not unincumbered with inconſiſtencies, which I do not deem it neceſſary to notice in this place. His aſſertion "that the white men of the Miſſouri ſpeak pure Welch," even though this aſſertion be qualified by the obſervation that "they occaſionally make uſe of a few words with which he was not acquainted," is to me one of the moſt improbable things that have ever been related of theſe people. His ſilence about their religion is altogether inexcuſable. One would ſuppoſe that a perſon of Griffith's inquiſitive turn of mind, would hardly have omitted to make ſome inquiries reſpecting the religious inſtitutions of a people, whom he conſidered as his countrymen. If theſe people be the deſcendants of Madog, *ſome* traces of the Chriſtian religion may be expected to be diſcerned among them; for I think it requires many centuries to entirely efface from the memory of a people all veſtiges of their religion, eſpecially from a people ſo tenacious of their language, and ſo little diſpoſed to intermix with their neighbours, as the Welch Indians are repreſented to be.

But Griffith's relation is, I think, worthy of ſome attention. I even think it not altogether improbable that future reſearches will eſtabliſh the fact, that there does exiſt in the weſtern parts of North America a race or nation of men, whoſe complexion is much fairer than that of the ſurrounding tribes of Indians, and who ſpeak a language abounding in Welch or Celtic words. But the *complete* eſtabliſhment of theſe two points would not prove the eſtabliſhment of the truth of the aſſertion, that Prince Madog had ever made a voyage to America, or that

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that a colony of Celts had at any period prior to the *diſcovery of America by Columbus*, paſſed into this hemiſphere *from Britain*.

It may be thought, from the ſtatement publiſhed by Dr. Williams and ſome other writers on the ſubject, that the belief of the exiſtence of a race of Welch Indians in America is generally admitted by the Welch Indians and others. But this is far from being the caſe. The late Mr. M'Gilivray, a man of no inconſiderable powers of mind, and whoſe curioſity was by no means confined to his own relatives, the Muſcohge, or Creek Indians, informed me, in the year 1790, that he knew nothing of the exiſtence of any white people in the tract of country beyond the Miſſiſſippi.

The following is an extract of a letter (dated *Downing, June* 14, 1792) from my learned and excellent friend the late Mr. Thomas Pennant of Wales.

" My countrymen are wild among the Padoucas, or Welch Indians, deſcendants of Madog, now ſeated about the upper parts of the Miſſouri. I am rather in diſgrace, not having the warmeſt hopes of their diſcovery. Pray what is your opinion and that of your philoſophers ?"

In anſwer of the above I wrote a letter, of which the following is a part:

" I have heard a great deal about the Welch Indians. I very early imbibed your opinion, as delivered in your Arctic Zoology *, and mentioned you on the ſubject in a little work † which I publiſhed in England at the age of * * * *. I do not know whether you have ſeen that work. I do *not* mean to hint that it is worthy of your attention. I certainly think there is ſome foundation for the ſtory; but I have no doubt but the whole affair will turn out very different from a diſcovery of Madog's deſcendants in America.

" I have ſaid, that I think there is ſome ground for the ſtory. I ſhall explain myſelf. You know that many of the firſt viſitors of the new world were ſtruck with the reſemblance which

* See the introduction to the work, pages 263, 264.

† Obſervations on ſome parts of natural hiſtory; to which is prefixed an account of ſeveral remarkable veſtiges, of an ancient date, which have been diſcovered in different parts of North America. Part I. London, 1797.

ſubſiſts

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ſubſiſts between ſome of the American nations and the Jews. Some Hebrew words were found in this continent, as they have been every where elſe. The Americans were now ſaid to be the deſcendants of the Jews, and Adair laboured very hard to prove the matter in a ponderous quarto which few people read, becauſe it is big with ſyſtem and extravagance, though, indeed, it contains ſome curious and accurate matter. In like manner, in the languages of ſome of the American tribes there are found ſome words which are a good deal analogous to words in the languages of the ancient Celts. Wafer, who was a very reſpectable obſerver, if we conſider his occupation in life, mentions the coincidence he found between the language of the Indians of Darien and that of the Highland Scots; and I could produce inſtances of their coincidence. Some Greek words are alſo found in certain of the American languages. I would not ſtrain a point ſo much as ſome writers have, who mention the coincidence which ſubſiſts between the Greek *Theos* and the Mexican *Teotl.* The word *Potowmack,* which is the name of one of our great rivers, is a good deal like the Greek *Potomos* ‡. Theſe words (perhaps they are accidental reſemblances) have given riſe to ſome of the numerous theories which we have had concerning the peopling of this great continent: and I doubt not that ſome * * * * or perſon who underſtood the Welch language, finding Celtic words (a language ſpoken by the Welch) among the Americans, in the fulneſs of his zeal would bring his countrymen among the Padoucas, Apaches, &c.

" Such, I believe, has been the origin of this wonderful ſtory. I preſume, that, were an ignorant Highlander to viſit the Darien Indians, or ſome other American tribes, he would fancy himſelf among his countrymen, whom painting, expoſure to the ſun, &c. he might ſuppoſe had exalted or degraded to their preſent tinge. I lately converſed with an old Highlander, who ſaid, that the Indians ſpeak the Highland language. Some Highland words were mentioned by him;

* The Abbé Molina (in his Compendio de la Hiſtoria Civil del Reyno de Chile, &c. Parte Segunda, p. 334, 335.) has pointed out ſome very ſtriking inſtances of reſemblance between the Greek and Chileſe languages. He has alſo pointed out ſome reſemblance between the Latin and the Chileſe.—February 19, 1805.

* * * * one

**** one word **** I recollect, the word *teine*, which in the Highland language, he ſaid, ſignifies fire: now our Delaware Indians call fire *teriday*; the reſemblance in ſound is certainly not ſmall. The Celts have, undoubtedly, been very widely ſpread over the globe: I believe they exiſted in this country, and that their deſcendants are ſome of the preſent tribes *. That Celtic words ſhould be found among the Americans, when Celtic words are to be found almoſt every where elſe, is not I think to be wondered at."

IX.

Account of an improved Sheep-Fold, contrived and conſtructed by THOMAS PLOWMAN, *Eſq. of Broom in Norfolk, and communicated by him to the Society for the Encouragement of Arts* †.

Advantages of the new ſheep-fold.

THE model of Mr. Plowman's Sheepfold was forwarded to the Secretary of the Society of Arts laſt year with a letter deſcribing its properties and conſtruction. It is made on an improved and very ſimple principle, combining many advantages over the old and expenſive method of folding by hurdles; and as the whole fold can be removed with eaſe at all times, it is found peculiarly uſeful in feeding off turnips on the land in froſty weather, when hurdles cannot be uſed; and, as the ſaving of labour in agriculture is a leading object, he has no doubt of ſeeing it, in a very few years, generally adopted.

Durability.

The expence, in the firſt inſtance, will exceed that of hurdles, for the ſame given quantity of ſheep; but having had one in uſe nearly three years, he is ſatisfied the ſaving will be very conſiderable: for, before he adopted this method of folding, he loſt from thirty to forty nights folding in the year, owing to the land being hard in dry ſeaſons, ſuch as the

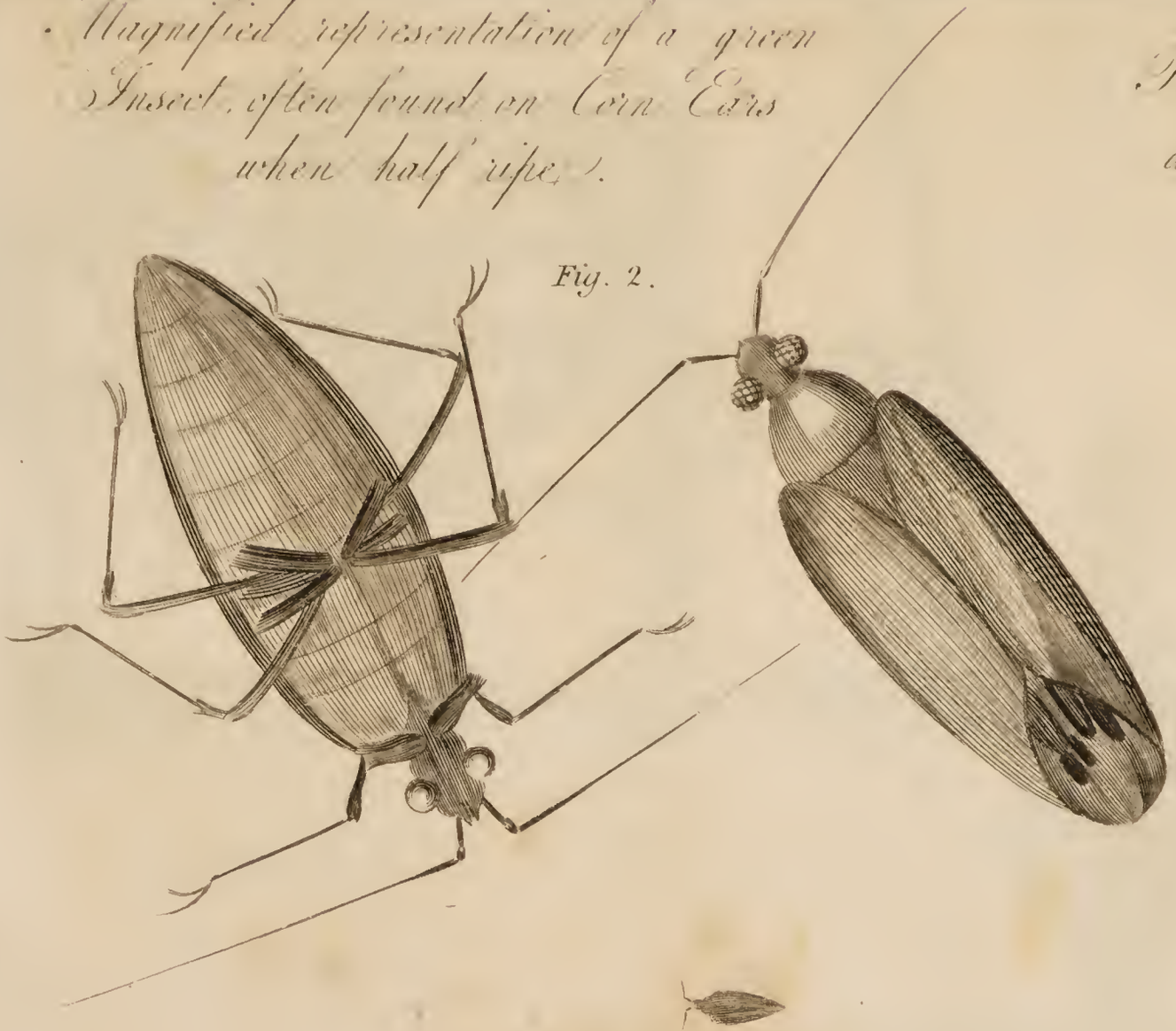
* Very conſiderable fragments of the Celtic dialects are ſtill preſerved in America; particularly, if I do not miſtake, among the Ranticokes and the Katalba or Katawbas. *February* 19, 1805.

† The Society awarded the gold medal for this uſeful improvement, and inſerted his account.

two

Magnified representation of a green Insect, often found on Corn Ears when half ripe.

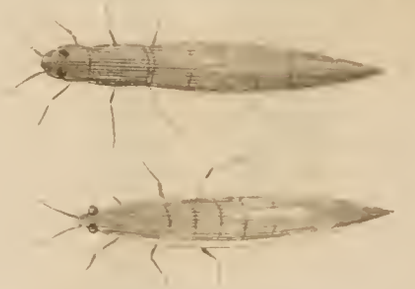
Fig. 2.



Natural Size.

Pale yellow Insect found in diseased Grain (magnified)

Fig. 1.



Natural Size.

Nicholson's Philos. Journal. Vol. XII. Pl. VIII. p. 224.

Mudow & Russell Sc.

Fig. 3.

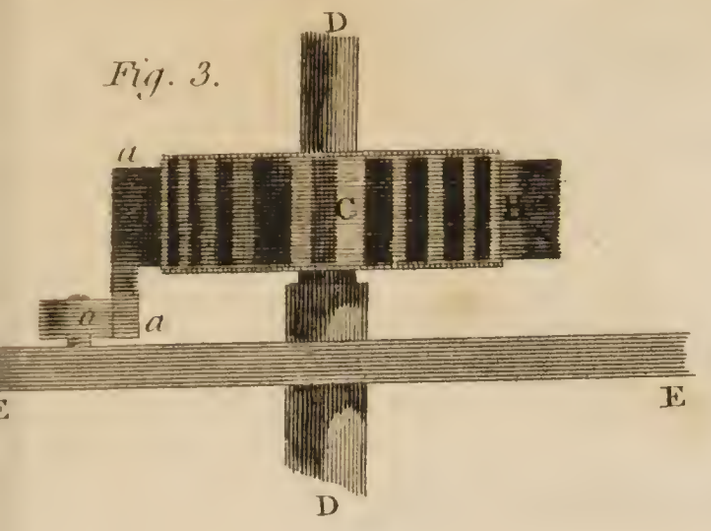
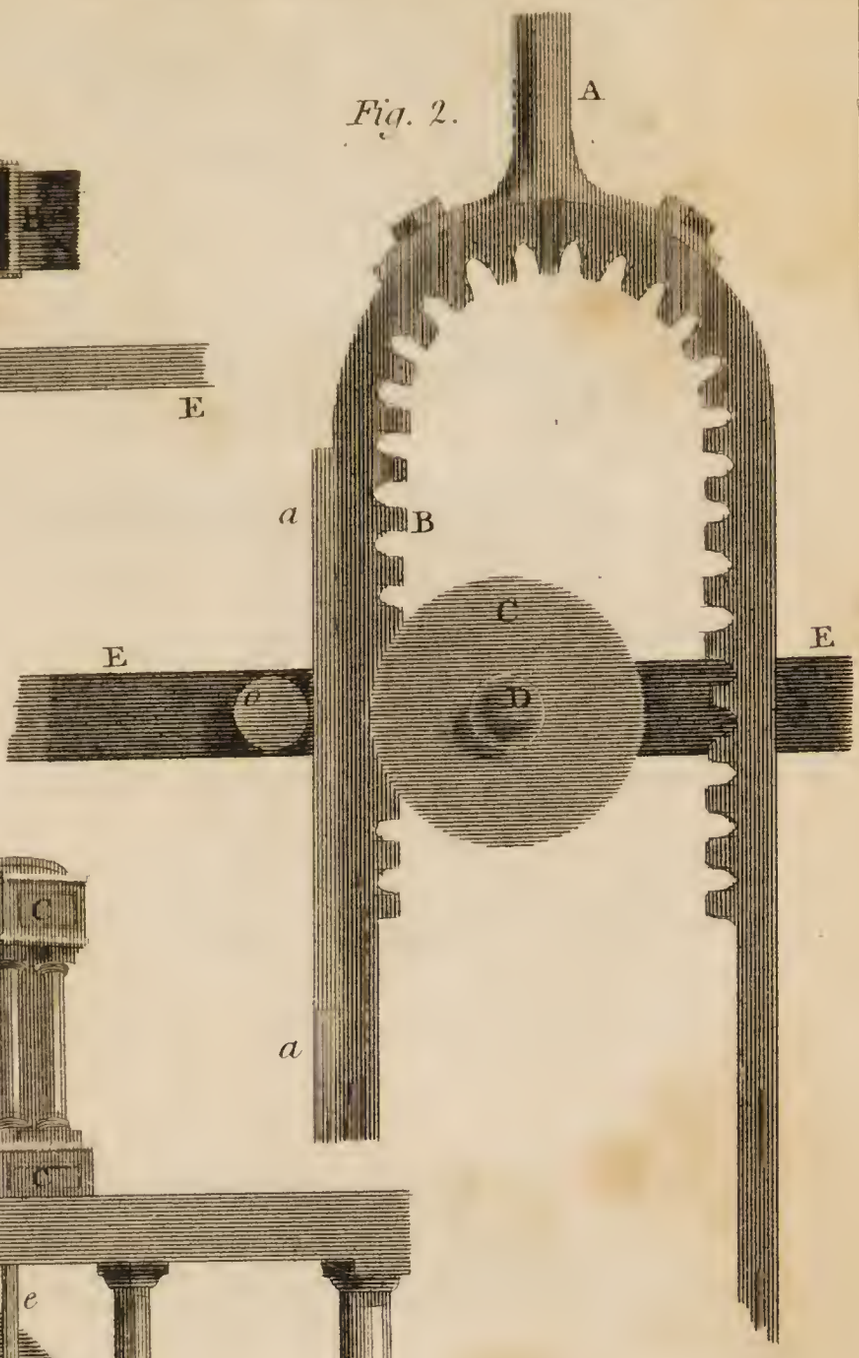
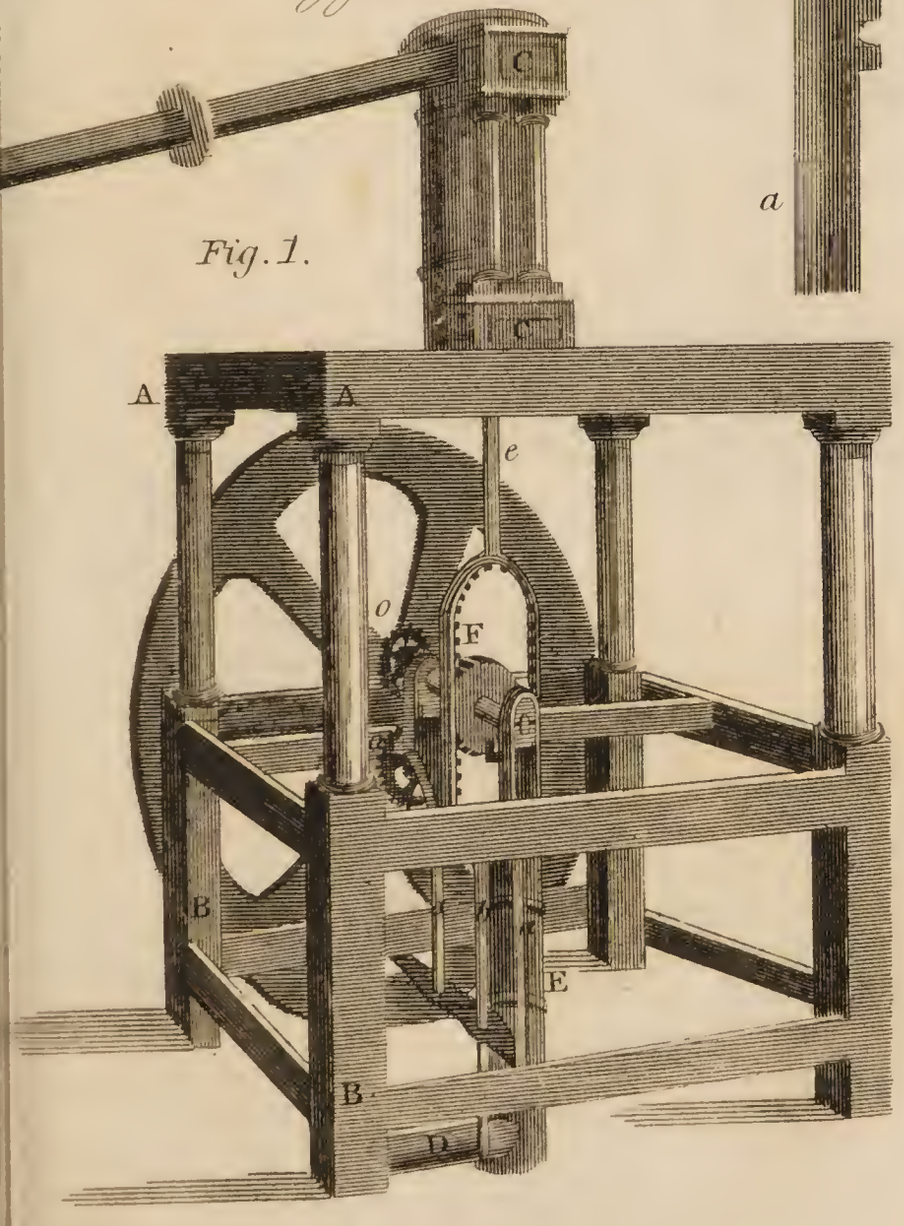


Fig. 2.



Portable Steam Engine; by Mr. Samel. Clegg.

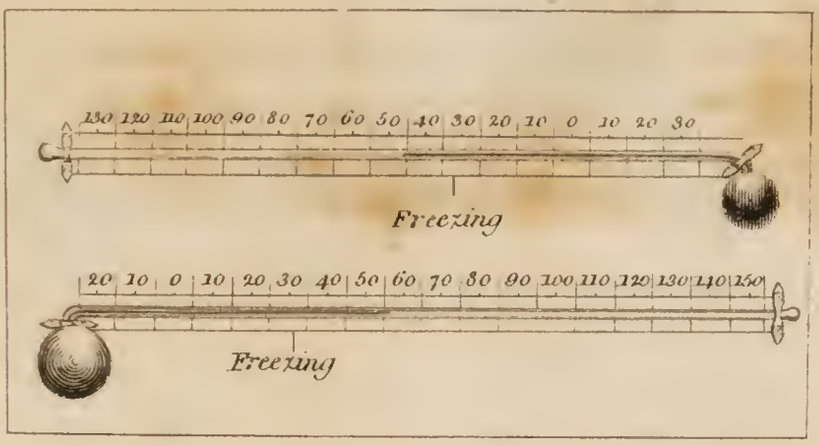
Fig. 1.



Mallow & Russell sc.

Self registering Thermometer.

Fig. 1.



Plan of Mr. Bloore's Truss, by which the Roof of Clapham Church was raised.

Fig. 2.

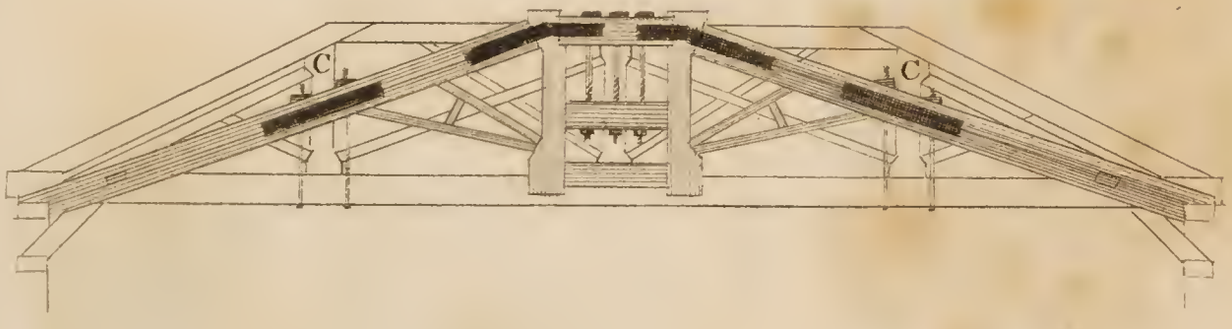
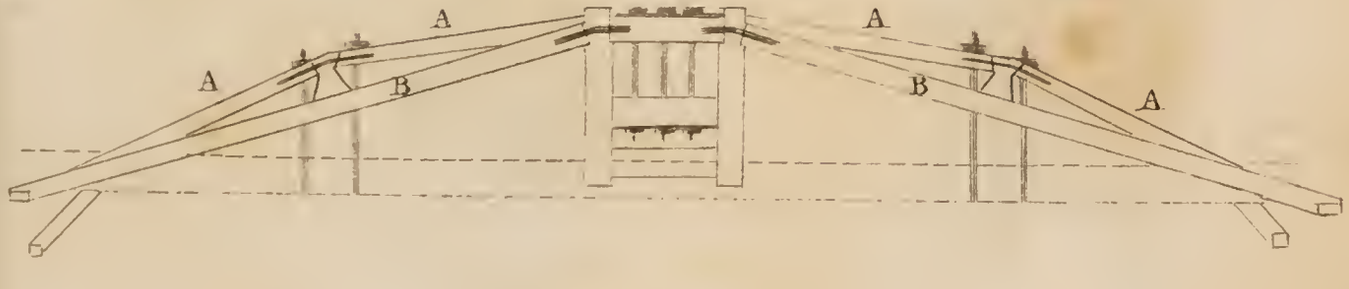


Fig. 3.



Improved Sheep-fold by J. Plowman Esq.r

Fig. 1.

Fig. 3.

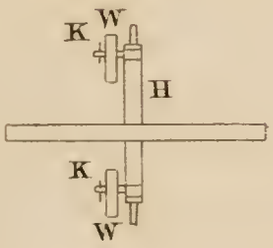


Fig. 2.

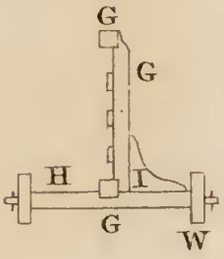
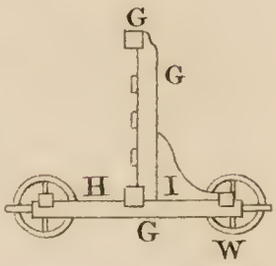
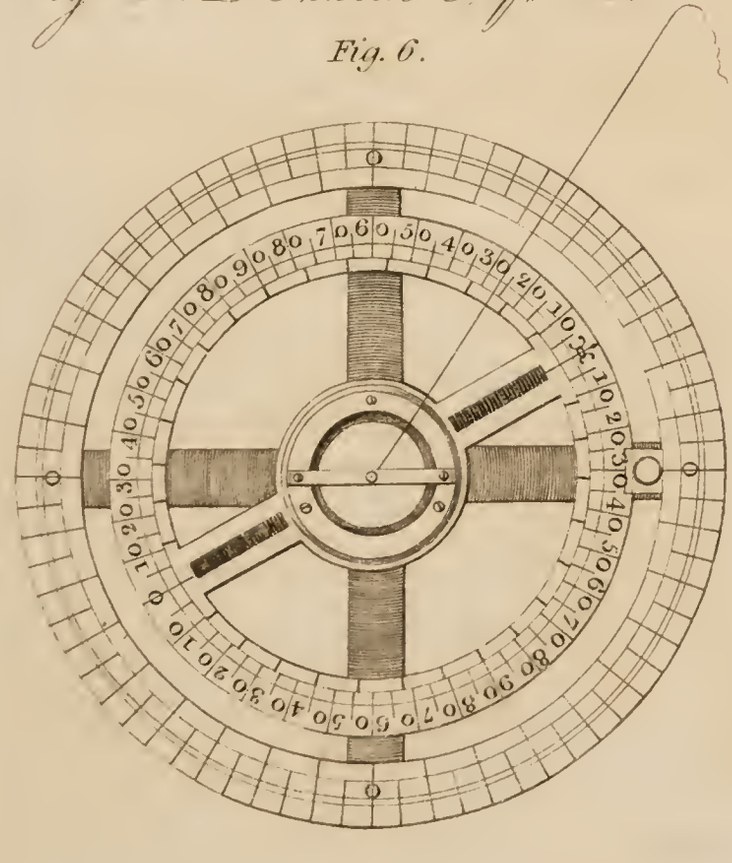


Fig. 4.



Applicative Compass by N. D. Starck Esq.r R.N.

Fig. 6.



Drain, by J. C. Curwen Esq.r

Fig. 5.

two laſt; which renders folding almoſt impracticable, as they never can be ſet without great labour and deſtruction of hurdles. He is alſo clearly of opinion, that the ſtock of ſheep will be greatly increaſed when this method of folding becomes more known; and that it will enable many ſmall farmers to keep from 50 to 100 ſheep, who now are deterred from it, on account of the ſmall quantity of feed they have, not anſwering to keep a man for that purpoſe only; but by this plan, they may keep a boy at 3s. or 3s. 6d. per week, who can attend on 100 or 200 ſheep, and move the fold himſelf without any aſſiſtance. In heavy gales of wind it frequently happens that hurdles are blown down, and the ſheep, of courſe, being at liberty to range over the crops, do incalculable miſchief; which cannot happen with this fold.

Saving of hurdles; and

greater profit in feeding and keeping ſheep.

It is eaſily moved, and not liable to be blown down;

In ſome counties in England, where hogs are folded, great difficulties are experienced for want of ſtowage, for them to feed off winter tares, &c. &c. as they root up every ſtake or hurdle; but from having tried the experiment, the inventor is certain his fold will keep them in, and defies their attempts to diſplace it.

reſiſts hogs.

From this drawing, which correſponds with the model, and from the deſcription, it is ſeen that an aſtoniſhing quantity of time is ſaved; for one man can remove a fold to contain 300 ſheep with eaſe in five minutes, which, by the old method, frequently takes ſome hours to accompliſh.

Certificates of gentlemen, who uſe theſe new folds, were ſent to the ſociety, among whom is that of his Grace the Duke of Bedford.

When the fold is wanted to be uſed on very hilly ground, it is beſt to begin at the top, and work it down to the bottom, for the eaſe of removing it, and then draw it up again with a horſe. This, however, the inventor has never had occaſion to do; for the land in his county is ploughed in a contrary direction, and the fold is worked in the ſame courſe as the ridges. By this mean, the inconvenience is avoided of croſsing the furrows, and they are alſo a guide to keep the fold in a ſtraight direction.

Method of placing it on hilly grounds.

With reſpect to the ſheep getting under, he does not recollect that circumſtance to have ever happened, nor does he conceive that any land, which is cultivated can be ſo uneven as to admit of it.

Description with reference to the drawing.

Description of the Sheepfold.

Plate XI. Fig. 1. Shows one division or part of this fence twenty-one feet long, and three feet eleven inches high, compoſed of the following parts:

A. A top rail three inches deep and two inches thick. B. The upper bar, three inches deep, and three-quarters inch thick. CC, The two lower bars, four inches by three-quarters of an inch, which, with the upper bar, are morticed through the uprights. DDDD, Which uprights are oak, three inches by two inches. E, The lower bar, three inches by three. F. An upright bar, with the horizontal bars halved into it. GG, Two oak uprights, three by two inches.

Fig. 2. Shows the oak uprights GG. H, The axletree, three inches by three, and three feet between the wheels. I, An oak knee, which connects the uprights G G with the axletree, by means of two ſcrews and nuts.

Fig. 3. A plan, in which the axle H is ſhown with two arms KK at right angles to H, which are made to act as pivots to the wheels, when intended to be moved in a direction at right angles to the bars.

Fig. 4. Is a view of the ſame parts deſcribed in fig. 3. The wheels marked W, in all the figures, are of caſt iron, and coſt 3s. 6d. each.

X.

Anecdotes of an American Crow. By William Bartram*.

Anecdotes of a crow.

It is a difficult taſk to give a hiſtory of our crow. And I heſitate not to aver, that it would require the pen of a very able biographer to do juſtice to his talents.

Before I enter on this ſubject minutely, it may be neceſſary to remark, that we do not here ſpeak of the crow collectively, as giving an account of the whole race, ſince I am convinced that theſe birds differ as widely as men do from each other in point of talents and acquirements, but of a particular kind of that ſpecies, which I reared from the neſt.

* From the Philadelphia Medical Journal, Vol. I. part I.

He

He was, for a long time, comparatively, a helpleſs dependant creature, having a very ſmall degree of activity or vivacity, every ſenſe ſeeming to be aſleep, or in embryo, until he had nearly attained his finiſhed dimenſions and figure, and the uſe of all his members. Then we were ſurpriſed and daily amuſed with the progreſſive developement of his ſenſes, expanding and naturating as the wings of the youthful phalæna, when diſengaged from its nympha ſhell. Anecdotes of a crow.

Theſe ſenſes however, ſeemed, as in man, to be only the organs or inſtruments of his intellectual powers, and of their effects, as directed towards the accompliſhment of various deſigns and the gratification of the paſſions.

This was a bird of a happy temper and good diſpoſition. He was tractable and benevolent, docile and humble, whilſt his genius demonſtrated extraordinary acuteneſs and lively ſenſations. All theſe good qualities were greatly in his favour, for they procured him friends and patrons, even among *men* whoſe ſociety and regard contributed to illuſtrate the powers of his underſtanding. But what appeared moſt extraordinary, he ſeemed to have the wit to ſelect and treaſure up in his mind, and the ſagacity to practice, that kind of knowledge which procured him the moſt advantage and profit.

He had great talents, and a ſtrong propenſity to imitation. When I was engaged in weeding in the garden, he would often fly to me, and after very attentively obſerving me in pulling up the ſmall weeds and graſs, he would fall to work, and with his ſtrong beak pluck up the graſs; and the more ſo, when I complimented him with encouraging expreſſions. He enjoyed great pleaſure and amuſement in ſeeing me write, and would attempt to take the pen out of my hand, and my ſpectacles from my noſe. The latter article he was ſo pleaſed with, that I found it neceſſary to put them out of his reach when I had done uſing them. But one time, in particular, having left them a moment, the crow being then out of my ſight, recollecting the bird's miſchievous tricks, I returned quickly and found him upon the table, rifling my inkſtand, books, and paper. When he ſaw me coming, he took up my ſpectacles and flew off with them. I found it vain to pretend to overtake him; but ſtanding to obſerve his operations with my ſpectacles, I ſaw him ſettle down at the root of an apple-tree, where, after amuſing himſelf for awhile, I obſerved

Anecdotes of a crow.

that he was hiding them in the grafs, and covering them with ſticks and chips, often looking round about to ſee whether I was watching him. When he thought he had ſufficiently ſecreted them, he turned about, advancing towards me at my call. When he had come near me, I ran towards the tree to regain my property. But he judging of my intentions by my actions, flew, and arriving there before me, picked them up again, and flew off with them into another apple tree. I now almoſt deſpaired of ever getting them again. However I returned back to a houſe a little diſtance off, and there ſecreting myſelf, I had a full view of him, and waited to ſee the event. After ſome time had elapſed, during which I heard a great noiſe and talk from him, of which I underſtood not a word, he left the tree with my ſpectacles dangling in his mouth, and alighted with them on the ground. After ſome time, and a great deal of caution and contrivance in chooſing and rejecting different places, he hid them again, as he thought, very effectually in the grafs, carrying and placing over them chips, dry leaves, &c. and often puſhing them down with his bill. After he had finiſhed this work, he flew up into a tree hard by, and there continued a long time talking to himſelf and making much noiſe; bragging, as I ſuppoſe, of his achievements. At laſt he returned to the houſe, where not finding me, he betook himſelf to other amuſements. Having noted the place where he had hid my ſpectacles, I haſtened thither, and after ſome time recovered them.

This bird had an excellent memory. He ſoon learned the name which we had given him, which was Tom; and would commonly come when he was called, unleſs engaged in ſome favourite amuſement, or ſoon after correction; for when he had run to great lengths in miſchief, I was under the neceſſity of whipping him, which I did with a little ſwitch. He would in general bear correction with wonderful patience and humility, ſupplicating with piteous and penitent cries and actions. But ſometimes when chaſtiſement became intolerable, he would ſuddenly ſtart off, and take refuge in the next tree. Here he would conſole himſelf with chattering and adjuſting his feathers, if he was not lucky enough to carry off with him ſome of my property, ſuch as a pen knife, or a piece of paper; in this caſe he would boaſt and brag very loudly. At other times he would ſoon return, and with every token of

penitence

penitence and ſubmiſſion approach me for forgiveneſs and reconciliation. On theſe occaſions he would ſometimes return and ſettle on the ground near my feet, and diffidently advance with ſoft ſoothing expreſſions, and a ſort of circumlocution, and ſit ſilently by me for a conſiderable time. At other times he would confidently come and ſettle upon my ſhoulder, and there ſolicit my favour and pardon with ſoothing expreſſions and careſſing geſticulations; not omitting to tickle me about the neck, ears, &c. Anecdotes of a crow.

Tom appeared to be influenced by a lively ſenſe of domination (an attribute prevalent in the animal creation) but nevertheleſs his ambition, in this reſpect, ſeemed to be moderated by a degree of reaſon or reflection. He was certainly by no means tyrannical or cruel. It muſt be confeſſed, however, that he aimed to be maſter of every animal around him, in order to ſecure his independence and his ſelf preſervation, and for the acquiſition and defence of his natural rights. Yet in general he was peaceable and ſocial with all the animals about him.

He was the moſt troubleſome and teazing to a large dog whom he could never conquer. This old dog from natural fidelity and a particular attachment commonly lay down near me when I was at reſt, reading or writing under the ſhade of a pear-tree in the garden near the houſe. Tom (I believe from a paſſion of jealouſy) would approach me with his uſual careſſes and flattery, and after ſecuring my notice and regard, he would addreſs the dog in ſome degree of complaiſance, and by words and actions; and if he could obtain acceſs to him, would tickle him with his bill, jump upon him, and compoſe himſelf for a little while. It was evident, however, that this ſeeming ſociability was mere artifice to gain an opportunity to practice ſome miſchievous trick, for no ſooner did he perceive the old dog to be dozing, than he would be ſure to pinch his lips, and pluck his beard. At length, however, theſe bold and hazardous achievments had nearly coſt him his life, for one time the dog being highly provoked, he made ſo ſudden and fierce a ſnap, that the crow narrowly eſcaped with his head. After this Tom was wary, and uſed every caution and deliberation in his approaches, examining the dog's eyes and movements, to be ſure that he was really aſleep, and at laſt would not venture nearer than his tail, and then by ſlow,

ſilent, and wary ſteps, in a ſideways or oblique manner, ſpreading his legs and reaching forward. In this poſition he would pluck the long hairs of the dog's tail. But he would always take care to place his feet in ſuch a manner to be ready to ſtart off when the dog was rouſed and ſnapped at him.

It would be needleſs (obſerves my ingenious friend in the concluſion of this entertaining account of the crow) to recount inſtances of this bird's underſtanding, cunning, and operations, which certainly exhibit inconteſtible demonſtrations of a regular combination of ideas, premeditation, reflection, and contrivance, which influenced his operations.

XI.

An Account of the Seiches of the Lake of Geneva.

By M. VAUCHER *.

Sudden and irregular riſe and fall of the lake of Geneva called ſeiches.

THE inhabitants of the banks of the lake of Geneva, deſignate by the name of ſeiches certain ſudden and irregular changes which take place in the level of the waters of the lake, and have no relation with the regular and annual increaſe produced by the melting of the ſnows. This phenomenon was deſcribed at the beginning of the laſt century. Fatio de Duilers in the 2nd vol. of Spon's Hiſtory of Geneva; and afterwards by Jalabert in the Academy of Sciences; Serre in the Journal de Savans, Bertrand, and by De Sauſſure in the 1ſt vol. of his Travels in the Alps. But though ſeveral of theſe philoſophers have attempted to explain the fact, as we ſhall hereafter remark, yet no one has conſidered it with preciſion, and as a general phenomenon. The editors of the Bulletin des Sciences, from whoſe excellent ſheet I take the preſent account, have followed Mr. Vaucher, and afterwards preſent the different explanations. The numerous obſervations of that philoſopher have led him to the following general reſults.

Particular detail of the facts; they are obſerved in other lakes.

1. The ſeiches are not peculiar to the lake of Geneva, they are alſo obſerved in the Lakes of Conſtance, Zurich, Annecy, Neuf-chatel, and in the lake Major, and there are ſtrong reaſons to think that they exiſt in moſt lakes, though they may not have been ſufficiently obſerved.

* From the Bulletin des Science, No. 96.

2. It

2. It appears, however, to be true, that the phenomenon is more remarkable in the lake of Geneva than any where elſe that it has been obſerved. In fact, the level of the waters of Leman lake have been ſeveral times obſerved to riſe at a given place in the courſe of 15 or 20 minutes, three, four, and even five feet, and to ſubſide ſome time afterwards, whereas the ſtrongeſt ſeiches obſerved in other lakes, have been four or five inches in the lake of Conſtance, eighteen lines in that of Zurich, four or five lines in that of Annecy, and only a few lines in the lake of Neuf-Chatel and lake Major.

But moſt ſtrikingly in the lake Leman.

3. In all theſe lakes, particularly in that of Geneva, the ſeiches are moſt ſenſible in that part of the lake which is neareſt the outlet of its waters. Accordingly they are no more than one or two inches, at the diſtance of two leagues from Geneva, and at the extremity near where the lake receives its waters the ſeiches of the lake of Geneva are not ſtronger than thoſe of the other lakes here mentioned.

More conſiderable near the place of efflux,

4. In theſe different lakes they are moſt ſenſible in places where the lake is remarkably narrow.

and where the ſhores are not far aſunder;

5. The ſeiches may take place indifferently at all ſeaſons of the year, and at any hour of the day; but in all the lakes it has been obſerved, that they are more frequent in the day than in the night, and in the ſpring and autumn, than in the winter or ſummer.

they happen at all times and ſeaſons;

6. It has been obſerved in particular in the neighbourhood of Geneva, that the ſtrongeſt ſeiches take place at the end of the ſummer, that is to ſay, at the time of the greateſt elevation of its waters.

but moſt ſtrikingly when the waters are higheſt;

7. The ſeiches are extremely frequent, but they are uſually a few lines, or at moſt only a few inches, in which caſes they cannot be perceived without exact apparatus to obſerve the level of the lake. It is from a want of this obſervation that they have been ſuppoſed to be very rare, as thoſe ſeiches only could be obſerved without apparatus which varied ſeveral feet.

8. The ſeiches take place without any agitation or motion of undulation or current in the ſurface of the fluid.

attended with no agitation,

9. Their duration is very variable, ſeldom exceeding twenty or twenty-five minutes, and often much leſs.

and do not laſt long;

10. This

they ſeem to be dependent on the weight of the atmoſphere, and are thought to foretel rain.

10. This phenomenon takes place in all temperatures, but in general it reſults from very extenſive tables, that the ſeiches are more frequent, and more extreme, the more variable the ſtate of the atmoſphere. Remarkable variations of the barometer have been obſerved to correſpond with conſiderable ſeiches, and it is an opinion generally received among the fiſhermen, that the ſeiches are a ſign of change of weather. In particular, they have been obſerved to be very ſtrong when the ſun comes to ſhine very ſtrongly on a ſpot, a ſhort time before obſcured by a thick cloud.

Explanations by various authors.

After this expoſition of the phenomenon, ſome notion may be formed reſpecting the value of the different explanations. M. Fatio attributes the ſeiches to violent guſts of wind which drive the waters into the narroweſt part of the lake. Mr. Jalabert attributes them to ſome ſudden encreaſe of the Arve, which falling into the Rhone at a ſhort diſtance from the lake, and entering the river at a conſiderable angle, may in fact, ſometimes ſtop its courſe for a ſhort period, and in that manner raiſe the waters of the part of the lake neareſt Geneva; laſtly, Mr. Bertrand thinks this phenomenon to be occaſioned by electrical clouds which attract the waters of the lake, and produce oſcillations more ſenſible, the nearer its oppoſite banks may be to each other. Without dwelling on the inſufficiency of theſe three hypotheſis to account for all the different facts before mentioned; Mr. Vaucher obſerves, that the true explanation ought to be two-fold; namely, general in order to ſhew the cauſe of thoſe leſs conſiderable ſeiches which are obſerved in all the lakes, and over the whole of their ſurface; the other muſt be local, and explain why this phenomenon is much more ſenſible at the weſtern extremity of the lake of Geneva, than in any other known place.

Mr. Vaucher aſcribes them to atmoſpheric preſſure acting more ſtrongly on one part of the lake than at the place of riſe.

With reſpect to the firſt, Mr. Vaucher aſcribes it to the frequent variations which are ſenſible in the weight of different columns of the atmoſphere, and conſequently in the preſſure it exerts on different points of the ſurface of lakes *. We may eaſily conceive, that if the weight of the atmoſpheric column be ſpeedily diminiſhed in a given part of a lake, with-

* This cauſe was before indicated concifely by De Sauſſure, in his firſt vol. of Travels in the Alps.

out

out the ſame thing happening over the reſt of the ſurface; or ſtill more if the weight ſhould be augmented upon that remaining ſurface, the water will be forced to riſe in that laſt place, and will again deſcend when the atmoſphere ſhall have reſumed its equilibrium. It is known, in fact, that theſe variations of the barometer are ſo frequent, that it can never be ſaid to be exactly ſtationary: it is known, that they can be produced by changes of temperature, and De Sauſſure has calculated that a diminution of three degrees in the column of air will account for a variation of 0,85 of a line in the barometer. It is known, that theſe variations are moſt frequent in mountainous countries in autumn and in ſpring, and previous to ſtorms, circumſtances which coincide with the greater frequency of ſeiches at thoſe times. This general cauſe tends to explain the ſlight variations of level which are common to all the lakes; it is even of ſuch a nature as to be applicable to all extended ſurfaces, and it is therefore probable, that theſe variations of level likewiſe take place in the ſea, independant of the flux and reflux, which may have hitherto prevented their being obſerved. The variations in the weight of the atmoſphere may perhaps contribute to thoſe ſudden and local elevations of the waters of the ſea, which have all been indiſtinctly conſidered as of the nature of water-ſpouts. The ſame cauſe ought likewiſe to act on rivers, but inſtead of raiſing or diminiſhing their level, it ought, according to Mr. Vaucher, to produce a momentary acceleration or retardation of their courſe; an obſervation difficult to be made, and not hitherto attempted.

—and he ſuppoſes the greater riſe in the lake of Geneva to be cauſed by its peculiar figure.

As to the ſecond part of the explanation, namely, that which ſhould account for the great intenſity of the phenomenon at the extremity of the Leman lake, near Geneva, Mr. Vaucher recurs to two circumſtances peculiar to that lake, and which are found in a leſs degree in thoſe of Zurich and Conſtance, where the ſeiches are moſt remarkable after thoſe of the lake of Geneva; namely, the contraction of a lake in a given place, and the deſcent of its waters towards the place of their diſcharge. With regard to the firſt of theſe circumſtances, it will be ſufficient, if attention be paid to a chart of the Leman lake, to ſhew that it is very remarkably contracted at its weſtern extremity, ſo that at half a league diſtance from Geneva, it has not one third of the breadth of that before Thonon. Now

we

we may compare a lake of this form to a ſyphon full of water, of which the branches ſhould very much differ in diameter; and it will be evident that if, for example, their inequality being as fourteen to one, the ſmalleſt branch ſhould ſuddenly receive by the augmentation of the atmoſphere a ſurcharge equal to that which depreſſes the barometer one line, it would fall 14 lines, and the water which would be driven into the great branch would raiſe its ſurface only one line; whereas, on the contrary, a ſurcharge which ſhould depreſs the level of the great branch only one line, would raiſe it for a moment fourteen in the ſmaller. The effect would be double if at the ſame time the weight of the atmoſphere ſhould diminiſh on one of the branches, and encreaſe on one of the other. We may therefore admit that in lakes, the breadth of which is remarkably contracted in ſome part, the influence of the variations of the atmoſphere to produce ſeiches will be greater in the narrow than in the wide part.

And alſo by circumſtances attending the flowing off of the waters.

A like effect will take place according to Mr. Vaucher, by reaſon of the inclination obſervable in that part of the ſurface of the lake near the place where it diſcharges its water. He remarks that every particle of a liquid on a ſlope may be conſidered as ſolicited by two forces; one which tends to raiſe it to the level of the ſuperior part of the ſlope or the reſervoir, and the other which urges it in the direction of the current. If by the ſudden depreſſion of the ſuperior fluid the current be for a moment ſuppreſſed, the particle will no longer find itſelf urged but by the firſt of theſe forces, and will riſe towards its ancient level, and ſoon afterwards deſcend. Now, as we have before ſeen, all the parts of lakes which have very perceptible ſeiches have a remarkable ſlope; this ſlope is naturally more conſiderable at thoſe times of the year when the waters are higheſt, and theſe are the periods when the ſeiches are moſt ſtriking in the neighbourhood of Geneva.

Singular appearance which ſometimes occurs that the ſurface of the lake is partly ſmooth and partly agitated.

Independent of the phenomenon of the ſeiches, the lake of Geneva and moſt other lakes afford two other ſingular phenomena; the one is known by the fiſhermen of the Leman lake by the name of fontaines. This takes place when the ſurface of the lake, inſtead of being uniformly calm or uniformly agitated, is ſeen to have certain parts calm and certain parts agitated, which are often mixed among each other in a thouſand manners, and always very diſtinct. This fact ſeems to indicate

cate the different atmoſpheric columns, though very near each other, may ſome of them be agitated and others calm. This appearance of the ſurface of the lake is conſidered by the fiſhermen as a ſign of rain.

The ſecond phenomenon of which Mr. Vaucher ſpeaks, conſiſts in certain ſonorous diſtant exploſions or noiſes which reſemble thoſe of the diſcharge of artillery, and are ſometimes heard in the fine ſummer evenings. This phenomenon is rare, but is nevertheleſs affirmed by ſeveral inhabitants near the lake of Geneva. It alſo takes place in the lake of Zurich according to Mr. Eſcher, and in that of Baikal according to that of Mr. Patrin. Mr. Eſcher aſſerts that half or three quarters of a minute after having heard one of theſe noiſes he ſaw a bubble of air about a foot in diameter riſe out of the lake of Zurich.

Another phenomenon reſembling the diſtant noiſe of artillery.

Annotations.—W. N.

It does not ſeem to me that any of the cauſes yet pointed out are ſufficient to account for the effect of the ſeiches. Sudden or ſtrong blaſts of wind could ſcarcely operate in this way ſo partially as that the exiſtence of ſuch ſqualls ſhould not at the ſame time have fixed the attention of the common people as well as of the more accurate obſervers who have noticed theſe changes. It is perhaps equally difficult to ſuppoſe ſuch unheeded variations to take place in the Arve ſufficient to account for theſe very remarkable changes in the lake. Mr. Bertrand's electrical hypotheſis refers us to a claſs of appearances too little underſtood to be admitted, otherwiſe than in the way of looſe conjecture; beſides which, it muſt be remarked that the agency of electrical clouds is much more generally directed to mountains than to the valleys in which lakes muſt neceſſarily have their ſituation. Much ingenuity is laſtly ſhewn by Mr. Vaucher in his explanation, which nevertheleſs requires us to admit of atmoſpheric columns conſiderably differing in weight and occupying very ſmall extent of ſurface. If this be even admitted as poſſible, yet ſtrong doubts may ſurely be entertained as to its probability. It appears to me that the object in queſtion admits of an eaſy ſolution upon other principles, and alſo that his explanation is grounded on poſitions not conſiſtent with the known laws of ſtatics.

Objections to the theories which have been offered reſpecting the ſeiches;

—particularly that of Mr. Vaucher's of atmoſpheric preſſure.

This ingenious author aſſumes as the conditions of his general theory that the lake ſhould conſiſt of two portions of water,

Recapitulation of his facts and deductions.

water, one much more extenſive than the other, and connected by a narrower portion or gut. He then ſtates that if the atmoſpheric preſſure be greater upon the larger ſurface than on the ſmaller, the firſt will be depreſſed and the latter will riſe, and that the difference of elevation in each ſurface occaſioned by the paſſing of any given quantity of water will be greater the ſmaller the ſurface.

No atmoſpheric change can make a greater alteration in the lake than the correſpondent riſe and fall of a water barometer, which is much leſs than really takes place.

This is very true; but it can in no caſe happen, that the difference between the level of one water and the other can amount to a greater quantity than that of a water-barometer, by a like change, namely, about fourteen lines for every line of variation in the common barometer. That is to ſay, if the barometer were to riſe and fall again through half an inch, in the ſhort time of a ſeiche, which I believe ſcarcely if ever happens, the ſeiche itſelf could not riſe above ſeven inches. The whole range of variation in the barometer could only cauſe a riſe of three feet and a half inſtead of five which ſometimes happens.

Another theory offered; that the ſeiches depend a together on the rapidity of ſupply and facility of diſcharge.

I would venture to conjecture that this phenomenon is one among the numerous oſcillatory proceſſes which take place when two variable natural powers are oppoſed to each other in the production or modification of any event. Moſt ſmall lakes are formed by the enlargement of a river, by which the lake is ſupplied at one end and evacuated at the other. The quantity of water in the lake itſelf will, in theſe circumſtances, be always more than would be ſufficient to fill its capacity, taken from the level of the loweſt point of diſcharge. How much more it may be than this quantity will depend upon the ſtreams which enter and paſs out. An increaſe in the quantity of ſupply will keep the level higher, and ſo likewiſe will any increaſe in the obſtacles to its flowing off; and on the contrary, if the ſupply be diminiſhed, or if the facility of diſemboging be encreaſed, the level will be depreſſed. Theſe effects will take place moſt ſtrikingly at firſt at that end of the lake where the efficient cauſe operates. When any change has once taken place, ſuch as that of the depreſſion, it will continue for a ſhort time after the cauſe has ceaſed to act; ſo that the depreſſion would itſelf be followed by a riſe, even if the circumſtances which cauſed it were not alſo ſubject to a like variation.

This effect is ſeen in brooks and mill-ponds.

Changes of this kind, on a ſmall ſcale, are obſervable in mill-dams, and even in the ſmooth places in brooks or rivulets, as may

may be obſerved by taking notice of ſome part of the bank where a gently riſing ſand may render the changes of level more conſpicuous. The variableneſs of the weather at the ſpring and autumn, by occaſioning more frequent changes in the quantities of water, and conſequently in the ſtate of the rivers above and below the lakes in queſtion muſt render the ſeiches more frequent and extreme at theſe times. They will alſo be moſt evident at the ends of a long lake; and the other circumſtances will be modified by events that for the moſt part would require to be aſcertained by obſervations of the local circumſtances and events on the ſpot.

It muſt be more conſiderable in ſpring and autumn.

The diſtinct portions of rough and ſmooth ſurface called *fontaines*, which are obſerved on the lakes, are very ſtrikingly ſeen at ſea whenever a breeze ſprings up after a dead calm. This effect is very remarkable, and may perhaps be accounted for on the ſuppoſition that the incipient motions of the air may be attended with eddies that may act more ſtrongly on one part of the ſurface than another. This however does not ſeem reconcileable with a certain ſteadineſs of appearance with which the ſmooth and rough ſurfaces continue diſtinct from each other for certain lengths of time. I am not much ſatisfied with the conjecture which offered itſelf to me, or which may have been mentioned by ſome other perſon when I was at ſea many years ago; but it at leaſt deſerves to be noticed here. It is well known that the wind ſcarcely takes hold of water which is covered with any oily film, and from the experiments of Franklin and others we have learned that a ſingle drop of oil will rapidly ſpread over a large ſurface of water, and cauſe all the ſmall primary waves to ſubſide, rendering the ſurface extremely ſmooth. It ſeemed to me not unlikely that oily matter from animal remains might riſe to the ſurface of the ſea during a calm and ſpread itſelf irregularly over certain parts, which would continue ſmooth for a conſiderable time after the light commencing breeze had ruffled the other parts. I think from recollection that this appearance could not have laſted more than a quarter of an hour; but it is very common, and I often ſaw it. May not a ſimilar cauſe produce the appearance in the lake of Geneva.

The diſtinct patches of ſmooth and rough ſurface on the lake are very uſual at ſea after a calm.

Suppoſed to ariſe from oily matter on the ſurface of the water,

—of the lake alſo.

The ſonorous reports reſembling diſcharges of artillery ſeem very likely to ariſe from the extrication of gas at the bottom of the water, which riſes and breaks at the ſurface. I have no

The ſonorous reports reſembling artillery ſuppoſed to be made by gaſes.

no remark to make on this ſubject, but advert to it principally with a view to mention an effect not generally known, but calculated to ſhew the agitation which a ſmall quantity of aſcending air can produce in water. If a ſwimmer fill his lungs with air by inhaling as much as poſſible, and then dive down or deſcend into the water to the depth of fourteen, twenty or more feet, and when at that depth ſlowly blow the air out of his mouth, he will himſelf hear a roaring noiſe, and the ſpectators will ſee with ſurpriſe the ſurface of the water raiſed into a round or conical maſs about a yard in height, with the water flowing round on all ſides over a ſurface of ſeven or eight ſquare feet. I have little doubt but that the noiſe of this riſing column of water with the breaking of the bubbles of air would be very remarkable in one of the ſtill evenings or nights of ſummer, when the effect of noiſes is remarkably more impreſſive than when the louder ſounds of the day render them leſs obſervable, and in many inſtances altogether inaudible.

Remarkable effect of agitation in water, produced by air blown ſlowly through the lungs at the depth of ſeveral feet beneath the ſurface.

XII.

Experiments to aſcertain the beſt Colour for marking the Heads of Pieces of Cotton or Linen in the rough, which ſhall be capable of reſiſting the Operations of Bleaching, as well as the moſt complicated Proceſſes of Calico Printing, without ſpreading beyond the Limits of the Impreſſion. By Mr. HAUFFMANN.*

IN order that a colour may be proper to mark piece goods of every kind it is requiſite that it ſhould contain no ſubſtance or drug capable of ſolution in alkalies; it is equally neceſſary that its component parts ſhould not become white by oxygenation, and that they ſhould remain inſoluble in acids ſufficiently ſtrong for the bleaching proceſſes, as well as for the operations antecedent to the calico printing.

Properties required in a good marking colour or ink.

Colours compoſed of drying oil cannot therefore, as I have found, be uſeful in theſe kind of marks, becauſe they are not only attacked by alkaline and ſoapy liquids, but likewiſe becauſe they dry ſlowly, and by ſpreading beyond the limits of impreſſion, very often occaſion ſpots.

Oil colours are bad becauſe they yield to alkalies, &c.

* Annales de Chimie, LIII. 208.

If the colours of ſpirituous varniſhes were not ſubject to the inconvenience of too ſpeedy evaporation and drying they would be inadmiſſible on another account, namely, that the turpentine and reſins are eaſily converted into ſoap. Gum copal is equally unfit for marking colours, becauſe it quits the piece by ſimple ebullition in water. But as the varniſh which I have made defends veſſels of copper or any other metal from the action of acids of a certain ſtrength as well as from that of the atmoſphere, I have thought it might not be unacceptable to deſcribe its compoſition in this place. To obtain this varniſh from copal as pale and as clear as water, this gum muſt be reduced to very fine powder and expoſed with twelve parts of the fineſt oil of turpentine for ſeveral days, or until it ſhall be completely diſſolved at a moderate heat on a ſand bath in a capſule of braſs, ſtone ware, or porcelain, taking care to ſtir it as often as poſſible with a rod of glaſs. At the moment when the conſiſtence of ſyrup begins to take place, the entire ſolution of the copal is effected by agitation, particularly if a ſmall quantity of oil of turpentine be added from time to time to ſupply the loſs by evaporation. Three fourths of the oil of turpentine which is loſt by evaporation when open veſſels are uſed, may be ſaved by performing the proceſs in a long necked mattraſs, which is to be expoſed on a ſand bath a ſufficient time to complete the ſolution of the copal, and ſhaking it very often. The varniſh obtained by either of theſe methods becomes yellowiſh if the heat be urged too ſtrongly; and as by its too glutinous conſiſtence it would be difficult in its application, it is convenient, inſtead of diluting it with oil of turpentine, to mix it with one fourth or one fifth part of its weight of alcohol, taking care not to uſe too much, becauſe an exceſs would render it of a milky white by the precipitation of part of the copal, which cannot admit in its ſolution more than a certain quantity of alcohol without precipitating. Veſſels of braſs or of any other metal may receive one, two, or three coatings of this varniſh, and muſt be each time well dried in the oven. After this treatment they may be waſhed with boiling water without injury, and may even be expoſed to a ſtill greater heat without the varniſh coming off; but theſe veſſels muſt not be rubbed with ſand or other hard bodies.

Varniſh colours are equally faulty in this reſpect.

Copal yields to boiling water.

Proceſs for making a good varniſh. Copal in powder is diſſolved by heat and agitation in oil of turpentine.

The copal varniſh is to be diluted with alcohol.

Metallic veſſels having this varniſh baked upon them may be expoſed to boiling water without injury.

By means of oil of turpentine, which evaporates and dries leſs ſpeedily than alcohol, I ſucceeded in making a black compoſition

An oily compound for marking linen and other goods.

poſition, which I expected might be uſed with advantage in marking piece goods. For this purpoſe nothing more is needful than to diſſolve ſlowly on the ſand bath, and with conſtant agitation. One fourth of its weight of aſphaltum or bitumen, judaicum well pounded, and afterwards to mix as much lamp black, or any other dark coloured mineral in fine powder, ſuch as black lead, galena, or the like. This colour may be had more or leſs thick, by due proportions of the oil of turpentine and bitumen; it prints very well without running, if the proper proportions be attended to, and a little oil of turpentine be added when it becomes too thick. This bituminous colour ſupports the action of alkalies and of oxigen very well, and reſiſts all acids of moderate ſtrength.

As I thought it unneceſſary to continue my experiments on oil colours, I made my experiments on watery compounds in the following order.

Section I.

First marking proceſs. An impreſſion is made of a ſolution of ſulphate of manganeſe thickened with gum, and covered with lamp black, the cloth being then dipped in alkali, the manganeſe precipitates in brown oxide which affords a mark not to be diſcharged by bleaching, or by the printing proceſſes.

I diſſolved in four ounces of water one ounce of the ſulphate of Manganeſe without its water of cryſtallization; that is to ſay, it was in the ſtate it poſſeſſes when oxigen gas is procured from the black oxide of manganeſe, by means of the ſulphuric acid, and by raiſing the heat to ignition at the end of the proceſs. This ſolution was thickened with one dram of fine gum adragan in powder, and coloured with lamp black, in order to diſtinguiſh exactly the impreſſion which may be eaſily made with this black ſaline metallic maſs, of which nevertheleſs, we cannot make effective uſe without plunging the end of the marked piece into an alkaline ley, taking care that it ſhall not firſt be wetted with water, which would carry off the ſaline matter. The ley may be made with potaſh or ſoda, in the proportion of one part alkali to nine or twelve parts water. It may be uſed in the ſtate of carbonate, or rendered cauſtic with half a part of quick lime. The precipitation of the oxide of manganeſe from the marks by either of theſe alkaline ſolutions will take place (excluſive of the ſtain from the lamp black) of a yellowiſh white colour, which will become more and more brown by attracting the oxigen of the atmoſphere. The change of theſe marks to the brown, and even to a deeper colour inclining to black, will

will take place very ſpeedily by bleaching with the oxigenated alkaline muriatic ley, the pieces of which the ends have been plunged for a few minutes in the alkaline as before preſcribed. Theſe marks of the brown oxide of manganeſe reſiſt not only all the bleaching materials, and all acids of a requiſite force, but likewiſe the moſt complicated proceſs of manufacture of calico printing.

Section II.

The acetate of manganeſe cannot be uſed without the ſame manipulation, and as it is more coſtly it muſt be rejected.

If the acetic acid had not a much ſtronger affinity with manganeſe than it has with iron, and if it diſengaged itſelf as readily from the acetate of manganeſe as it does from the acetic ſolution of iron by evaporation and drying, we ſhould be able to procure indeliable marks in the moſt ſimple manner, by depoſiting the oxide of manganeſe on piece goods by means of the acetic acid, and afterwards ſimply leaving the oxide to the attraction and ſaturation of oxigen from the air. The acetic ſolution of manganeſe is very readily obtained by mixing a proper quantity of acetate of lead in a ſolution of ſulphate of manganeſe. But as this acetic ſolution affords no advantage in marking piece goods beyond thoſe of the ſulphate of manganeſe, and as it requires preciſely the ſame management as that deſcribed in the laſt ſection, and it is likewiſe more expenſive, it deſerves to be rejected.

Section III.

Sulphate of manganeſe with acetate of iron treated as before. It dries more quickly.

Two ounces of ſulphate of magneſia diſſolved in eight ounces of the acetic ſolution of iron, concentrated to the point indicated by twenty degrees, afford when thickened with one fortieth part of gum adraquack, a deep yellow liquor which becomes more and more brown, when treated abſolutely in the ſame manner as deſcribed in the firſt paragraph. The acetic ſolution does not, however, afford any other advantage but that of cauſing the marks to dry a little more ſpeedily; for the oxide of iron diſſolves in acids accordingly, as it is oxigenated. I give the preference to gum-adragant for thickening colours, to other gums and to ſtarch, becauſe theſe ſubſtances weaken the colours too much, if however, there ſhould be any objection to gum-adragan in coarſe goods, ſtarch may be then uſed.

Section IV.

Marks printed with the grey oxide of manganese obtained in washing the residual sulphate, afford fixed marks by simple impression.

If care be taken in the process of disengaging oxigen gas from a mixture of the black oxide of manganese and sulphuric acid, not to carry the fire to ignition, the saline residue remains blackish, instead of becoming yellowish white by strong heat. When this residue is dissolved in water, it leaves behind it an oxide of a deep grey, which acquires a very pasty consistence on the filtre. This oxide mixed with a very little water thickened with gum adragant, may be used to print marks of a very deep grey, which dries speedily; and this colour does not wash out with water, even though the subsequent dipping in an alkali be omitted. It is so fixed that it not only supports the action of all acids of the manufacturing strengths, but likewise all the bleaching and printing processes without attracting the colouring matter of any dye whatever.

Section V.

Addition of the nitro muriate of tin to the marking oxide. It affords a dye.

If there were no reason to fear injuring in a slight degree the place where the mark is made, it would be advantageous to employ equal parts of the last described grey paste, and of a nitro muriatic solution of tin, containing one fourth part of the metal, and thickened with gum-adragath. This colour is as unalterable as that of the fourth section; and it has the additional advantage, that its oxide of tin being saturated with oxide of oxigen, attracts the colouring parts of any tincture, and acquires a puce colour by madder. I must observe on this occasion, that by the same madder dye, the colours of marks from the oxide of manganese saturated with oxigen, become of a deep puce colour, inclining to black, whereas in a less oxigenated state they acquire fainter shades. In all these circumstances however, it is requisite, that the quantity of metallic oxide should be as great as possible, otherwise the shades will be various, and less intense.

Section VI.

Experiments with the precipitate of manganese and solution of iron.

As many insoluble metallic oxides acquire the property of adhering to stuffs by means of acid, I did not fail to try whether the same would be the case with the precipitate of manganese saturated with oxigen. For this purpose I dissolved one part,

of

of ſulphate of manganeſe in ſix parts of water, and precipitated the metal by adding to the point of ſaturation a cauſtic alkaline ley, made with half a part of quick lime, four parts of water, and one part of calcined potaſh of the ſhops. The precipitate was yellowiſh white. To the whole aqueous maſs I then added a ſufficient quantity of oxigenated muriatic alkaline ley, until the precipitate was completely ſaturated with oxigen, and its brown colour became no deeper. I afterwards collected on a filtre the precipitate or brown oxide of manganeſe, where, by the drainage of its water, it became paſty. This brown paſte, mixed with half its weight of the moſt concentrated acetic acid no longer afforded any but a weak brownish ſhade; it was the ſame with a ſmall addition of one or the other of the three ancient mineral acids in a ſtate of ſolution. I did not ſucceed better by mixing one part of the ſame brown paſte with an equal quantity of the acetic ſolution of iron, marking 20° of the areometer of the ſaltpetre makers and thickened with gum adraganth. This acetic ſolution of iron containing only the quantity of oxigen neceſſary for the ſolution of the metal ceaſed by a ſtronger affinity, the exceſs of oxigen of the brown oxide of manganeſe, which in its turn became diſſolved, and the mixture of the two metallic ſolutions afforded a yellow reddiſh very deep and tranſparent liquid, which confirms the fact that a metal ſaturated with oxigen requires leſs acid for its ſolution than if it were in an oppoſite ſtate, and that being then furniſhed with an exceſs of acid, the ſolution ſaturated with oxigen can admit a portion of another metal without becoming turbid. This mixed ſolution of the two metals afforded me only a ruſty yellow which was diſcharged by weak ſulphuric acid completely, in ſomewhat leſs time than was required to take out a ruſt ſpot in a leſs oxigenated ſtate. In order to obtain from the mixture of theſe two metallic ſolutions an indelible marking colour, it is neceſſary that the marks ſhould be ſteeped for ſeveral minutes in an oxigenated muriatic alkaline ley, to precipitate and ſaturate the oxigen of the oxide of manganeſe. By mixing half a part of the brown paſte of manganeſe to two parts of the ſolution of the two metals the new portion remains untouched and renders the whole turbid. This turbid mixture left only a light brownish mark on piece goods, which had remained long in the diluted ſulphuric acid.

Experiments with the precipitate of manganeſe and ſolution of iron.

The muriatic ſolution of tin takes up the oxides of iron and of manganeſe.

By means of the muriatic ſolution of tin, which has the property of taking the oxigen from many vegetable, animal, and mineral ſubſtances, and which, on this account, is very uſeful in dying, as well as in calico-printing. We may diſcolour and diſſolve inſtantly the deepeſt oxide of manganeſe and of iron, which proves the preponderating affinity of tin towards oxigen beyond that of manganeſe or of iron.

N. B. There is no reaſon to object to ſteeping the marked goods in an alkaline ley; the operation is ſpeedily made without ſenſible loſs of potaſh or of ſoda, if the operation of lixiviating be immediately proceeded upon, for which the remainder of the ley may be uſed. And generally, if the practice be uſed which has been adopted for a number of years, of rendering the alcalies cauſtic with quick-lime, the ſaving will be conſiderable and with better effect.

XIII.

Note on the Formation of Water by mere Compreſſion; with Reflections on the Nature of the Electric Spark. By M. Biot.*

That oxigen and hidrogen may unite by preſſure.

It was ſome time ago that, in converſation with M. Berthollet on the nature and properties of heat, I communicated to him the perſuaſion I had, that the combination of hidrogen and oxigen gaſes might be determined without the aid of electricity, and merely by a very rapid compreſſion. This reſult appeared to me a conſequence ſo immediately following the obſervations already made on the heat diſengaged from air by compreſſion, that I thought it needleſs to aſcertain it in any other manner. But having ſince converſed with Mr. La Place, he appeared ſo intereſted as ſtrongly to urge me to a verification. I therefore made the experiment, which completely ſucceeded. It was made in the cabinet of the Polytechnic School. I am greatly indebted to M. Haſſenfratz, profeſſor of natural philoſophy in that eſtabliſhment, for the

* Read to the National Inſtitute of France, and inſerted in the Annales de Chimie. LIII. 321.

great

great attention he paid in cauſing the requiſite preparations to be made, and for his perſonal aſſiſtance in repeating it.

Experiment in proof. The ſyringe of an air-gun filled up ſo as to receive the gaſes and admit of inſpection into its chamber, was firſt tried with common air,

We took the ſyringe of an air-gun, the bottom of which was cloſed by a very thick glaſs, in order that we might obſerve the light diſengaged as uſual by compreſſion. This ſyringe was of iron; it had a cock on one ſide to introduce the gaſes, and its lower extremity on the ſide of the piſton was enveloped by a cylinder of lead, ſufficiently weighty to accelerate the fall and render the compreſſion more rapid. This apparatus was firſt tried by introducing atmoſpheric air; but though the experiment was made in the dark, no perceptible light was ſeen, probably becauſe the violent motion neceſſary for the rapid compreſſion, prevented the operater from looking ſo directly through the glaſs as to perceive the tranſient light which compreſſion diſengages, and which I myſelf had ſeveral times ſeen.

and afterwards with a mixture of oxigen and hidrogen. The glaſs was broken by ſtrong luminous exploſion.

Immediately after this trial a mixture of hidrogen and oxigen gaſes was introduced into the ſyringe, and a ſtroke was given. An extremely brilliant light appeared with a loud detonation: The glaſs bottom was driven out: The copper ſcrew which retained it in its place was broken; and the perſon who held the ſyringe had his hand ſlightly burned and wounded by the force of the exploſion.

Repetition with a metallic cap. The ſyringe burſt.

The experiment was repeated, by ſubſtituting a braſs bottom of one entire piece ſcrewed on inſtead of that of glaſs. and a new mixture of the gaſes was introduced. The firſt ſtroke of the piſton produced an exploſion, which was heard like the loud crack of a whip; but a ſecond ſtroke with a new charge of the gaſes, cauſed a detonation which broke or rather tore the body of the ſyringe with a violent exploſion.

After theſe phenomena there can remain no doubt reſpecting the combination of the two gaſes; as it is known that this combination produces the detonation by the immenſe quantity of heat diſengaged when they paſs to the liquid ſtate; a heat which is ſufficient to reduce them immediately into vapour, and give them an exceſſive dilatation in that ſtate. It was not therefore thought neceſſary again to repeat this experiment, which is attended with ſome danger.

Theory, the gaſes give out heat.

The theory of theſe phenomena is extremely ſimple. A rapid compreſſion forces the gaſes to abandon a very great

quantity of heat, which not being capable of immediate diſſipation, raiſes their temperature in the inſtant ſufficiently to inflame them in this ſtate of compreſſion.

Thus it is that we find in the two gaſes all the elements neceſſary for their combination, independently of the electric ſpark or external heat. We might probably in the ſame manner, and without any foreign agent, produce all the gaſeous combinations which require an increaſe of temperature.

Deduction. The electric ſpark may conſiſt merely of light driven from compreſſed air.

This identity of reſults has led me to a notion which I ſubmit to the judgment of philoſophers. It is known, and M. Berthollet has ſhewn it in his Chemical Statics, that electricity in paſſing through bodies, produces a true compreſſion of their particles. This effect is produced with the moſt extreme velocity, as may be proved by an affinity of experiments. Now electricity poſſeſſing a velocity ſo great, it is impoſſible that it ſhould not diſengage light from the air, ſince we can diſengage it by a compreſſion ſo much leſs rapid. In this way it is that we are led to a concluſion, that this reſult of the electric ſpark is the purely mechanical effect of compreſſion.

More ample explanation. The extreme velocity of electric matter will ſtrongly compreſs free air,

If we now compare what paſſes in our condenſing pump and in the eudiometer of Volta, we ſhall find that the analogy is complete. Only that in the firſt caſe we are obliged to confine the air, becauſe the velocity we can give to the piſton is limited. Whereas in employing electricity, the particles are compreſſed by a velocity ſo great that they can never withdraw themſelves with ſufficient ſpeed from its effort. Therefore the compreſſion may be equally well made in the open air, together with the diſengagement of light or the ſpark, which is its conſequence. But this effect is local; and if the gaſes be not ſuſceptible of combining together, ſhould after each explоſion return to their primitive dimenſions, they muſt immediately reſume in this dilation all the heat they had before diſengaged, ſo that there cannot be effected any laſting change in their conſtitution. This explains why no alteration has ever been ſeen in very pure unmixed gaſes, when ſubjected to the action of the electric ſpark.

and alſo the rare fluids in our vacuum.

This light which electricity diſengages from the gaſes by compreſſion, it muſt alſo diſengage from the more rarified gaſes, and on account of its extreme velocity, it muſt diſengage it even from vapours, when experiments are made under the

receiver

receiver of an air-pump or in the torricelian vacuum: For we can never form a perfect vacuum with our machines, and even in the tube of the barometer mercury always exists in the form of vapours. These vapours, though very rare, still contain a large quantity of caloric, which the electricity must disengage in its passage by compression; but the instantaneous augmentation of electricity which results, cannot become sensible on account of the little density of the medium; but this increase is perceivable in denser air, as we see in the instrument called Kinnersley's thermometer.

The considerations which I have here made, appear to me to point out with some probability, that the phenomenon called the electric spark, is owing to the light disengaged from the air by compression during the passage of the electricity; so that this phenomenon is purely mechanical, and not at all electric in itself. This is the notion which I submit to the judgment of philosophers: if it be true, it must tend considerably to diminish the number of hypotheses which have already been made, or may be made on the nature of electricity. For this reason it is that I have offered it to their consideration, requesting that it may not be be thought that I consider it as of greater importance than their deliberate examination may bestow upon it. Conclusion.

XIV.

Account of Thermometers for registering the highest and lowest Temperatures in the Absence of the Observer. *By* F. A.

To Mr. NICHOLSON.

SIR,

MANY contrivances have been proposed and adopted for registering all the stations of the thermometer and barometer, by means of a float or other equivalent instrument carrying a pencil, which marks its situation on a surface gradually moved along by means of a clock. These, of which meteorologists know the value, are nevertheless expensive, and require a degree of care and management sufficient to render simpler contrivances Thermometers and barometers for registering the weather.

Six's thermometer.

contrivances acceptable. Mr. James Six communicated, about 25 years ago, to the Royal Society a thermometer, in which two ſmall indicating pieces were driven by the fluid in the tubes to ſtations where they ſtuck, and remained after the change of temperature, and ſhewed the higheſt and loweſt degrees that had occurred ſince the laſt placing of them in contact with it. As this inſtrument is ſufficiently known, and I am now to advert to a ſimpler contrivance, I will diſmiſs that ſubject and advert to this laſt.

Objections to it.

In Mr. Six's complicated thermometer the tubes were vertical, and the indexes ſtuck in the glaſs by their ſpring; beſides which, a ſmall piece of ſteel wire being expoſed to alcohol, was at length oxided and ſet faſt. The other contrivance now to be ſeen in all our London ſhops, and reſpecting which you will do an acceptable ſervice to your readers and the ſcientific world, by inſerting a ſketch in your Journal, conſiſts ſimply in two thermometers, one mercurial and the other of alcohol (*Fig.* 1, *Pl.* X.) having their ſtems horizontal; and the former has for its index a ſmall bit of magnetical ſteel wire, and the latter a minute thread of glaſs, having its two ends formed into ſmall knobs by fuſion in the flame of a candle.

Another thermometer, which ſhews the greateſt and leaſt temperatures.

Deſcription. It has a mercurial thermometer which ſhews the maximum; and a ſpirit thermometer for the minimum.

The magnetical bit of wire lies in the vacant ſpace of the mercurial thermometer, and is puſhed forward by the mercury whenever the temperature riſes and puſhes that fluid againſt it: but when the temperature falls and the fluid retires, this index is left behind, and conſequently ſhews the maximum. The other index, or bit of glaſs, lies in the tube of the ſpirit thermometer immerſed in the alcohol, and when the ſpirit retires by depreſſion of temperature, the index is carried along with it in apparent contact with its interior ſurface: but on increaſe of temperature the ſpirit goes forward and leaves the index, which therefore ſhews the minimum of temperature ſince it was ſet. As theſe indexes merely lie in the tubes, their reſiſtance to motion is altogether inconſiderable. The ſteel index is brought to the mercury by applying a magnet on the outſide of the tube, and the other is duly placed at the end of the column of alcohol by inclining the whole inſtrument.

Queſtion. Why the ſmall glaſs index always remains in the ſpirit.

I beg you will explain the motion of the glaſs index. I can eaſily underſtand from the general fact *that mercury repels ſteel*, that this fluid will drive the ſteel index before it; but I cannot

make

make out to my ſatisfaction, how the ſpirit, by attracting glaſs, can prevent the other index from ever riſing out of its ſurface. Perhaps this thing may be already explained in elementary books; but whether it be or no, I am ſure that an account in your clear and popular way cannot be thought ſuperfluous.

I am reſpectfully,

Sir,

Your obliged

F. A.

REPLY.—W. N.

When the ſurface of the column of ſpirit is viewed by a magnifier, it is ſeen to have the form of a concave hemiſphere, which ſhews that the liquid is attracted by the glaſs. The glaſs in that place is conſequently attracted in the oppoſite direction by a force equal to that which is ſo employed in maintaining that concave figure; and if it were at liberty to move, it would be drawn back till the flat ſurface was reſtored. Let us ſuppoſe a ſmall ſtick or piece of glaſs to be looſe within the tube, and to protrude into the vacant ſpace beyond the ſurface of the alcohol. The fluid will be attracted alſo by this glaſs, and form a concave between its ſurface and that of the bore of the tube. But the ſmall interior piece being quite at liberty to move, will be drawn towards the ſpirit ſo long as the attractive force poſſeſſes any activity; that is, ſo long as any additional fluid hangs round the glaſs; or in other words, until the end of the ſtick of glaſs is even with the ſurface. Whence it is ſeen that the ſmall piece of glaſs will be reſiſted, in any action that may tend to protrude it beyond the ſurface of the fluid; and if this reſiſtance be greater than the force required to ſlide it along in the tube (as in fact it is), the piece muſt be ſlided along as the alcohol contracts; ſo as always to keep the piece within the fluid. And this fact is accordingly obſerved to take place.

Explanation. If the ſmall glaſs piece were protruded beyond the ſpirit, the fluid would hang to it and draw it back.

Abſtract

XV.

Abſtract of a Memoir on Milk. By M. THENARD.*

Component parts of milk.

IN a memoir which I read to the Philomatic Society in Praireal laſt, I ſhewed that milk always contains the free acetous acid in a greater or leſs quantity. At the ſame period Meſſrs. Fourcroy and Vauquelin found that it alſo contains phoſphate of magneſia, and that the lactic acid of Scheele, or that which is obtained from ſerum of milk ſpontaneouſly coagulated, is merely the acid of vinegar combined with an animal matter. So that in the preſent ſtate of our knowledge we muſt conſider milk as compoſed of, 1. Water; 2. Acetous acid; 3. Caſeous matter; 4. Butteraceous matter; 5. Sugar of Milk; 6. Extractive matter; 7. Muriate of ſoda and of potaſh; 8. Sulphate of potaſh; 9. Phoſphate of lime; 10. Phoſphate of magneſia.

Of theſe eleven ſubſtances there is one which I particularly examined ſome months ago, namely cream. I was deſirous of aſcertaining the circumſtances which govern its ſeparation, and particularly its transformation into butter.

The ſeparation of cream and of butter does not require acceſs of air.

I had before obſerved that milk coagulates as readily in cloſed as in open veſſels; I know that no gas is diſengaged in this decompoſition, and that, in order to effect it with rapidity, it is needful only to raiſe the temperature to between 20° and 40° (Reaumur I ſuppoſe; and, if ſo, anſwering to 77° and 122° Fahrenheit). It was clear, therefore, that the air contributes neither to the formation nor the ſeparation of cream, but that it exiſts ready formed in milk; but it remained to be ſhewn what are the principles which enter into its compoſition. Being perſuaded, from various obſervations I had made, that it is only an intimate mixture of butter, cheeſe, and ſerum, I proceeded to aſcertain this point by mixing a pint bottle (Engliſh quart) of recent cream nearly to its neck, from which I diſplaced the remaining air by carbonic acid. I then cloſed it well, and agitated it ſtrongly in every direction for half an hour; at the end of which time the contents having

* Soc. Philomath, No. 96.

become

become very thick and adhering ſtrongly to the ſides of the bottle, gradually became detached, and ſoon afterwards were converted into a white liquid, in the midſt of which ſwam a yellow maſs of excellent butter. Hence it follows, that the butter exiſts in the milk, and is ſeparated when the milk, being deprived of the vital action, is left to itſelf. At this time, either by the formation of an acid ariſing doubtleſs from a decompoſition of the extractive matter, or perhaps from the leſs ſpecific gravity of the butter compared with that of the cheeſe; for the butter begins to ſeparate almoſt at the moment that milk is poured into a veſſel;—the milk is decompoſed, the cream riſes to the top, and from this laſt, by agitation, and more particularly by the aſſiſtance of a temperature between 15° and 20° (66° to 77° F.), butter is obtained together with butter-milk, which is a white very mild liquor, in which ſome butter and cheeſe are ſuſpended in a very divided ſtate. But the butter thus obtained is not pure: It ſtill contains a portion of cheeſe amounting ſometimes to the ſixth part of its weight; and this is the cauſe of its ſpeedily becoming rancid, particularly in ſummer. When the cheeſy matter is ſeparated by fuſion, the butter may be kept a long time. It is true indeed, that by this fuſion it acquires an acridneſs which greatly limits its uſes, and makes it unfit to be employed in frying; but this diſadvantage might be remedied by keeping the temperature much lower than is uſual. Clouet firſt made this obſervation; and hence the following proceſs may be adopted for purifying butter, or ſeparating the cheeſy matter without giving it a bad taſte.

Proceſs of butter making.

1. Let the butter be melted on the water bath, or at a degree of heat not exceeding the 66° of Reaumur. 2. Keep it melted till all the cheeſy matter is collected in white flakes at the bottom of the veſſel, and the melted butter is tranſparent. 3. At this period decant it, or paſs it through a cloth. 4. Let it be cooled in a mixture of equal parts of pounded ice and ſea-ſalt; or if ice cannot be procured, then in cold ſpring-water, making uſe of broad ſhallow veſſels. Without this precaution the butter would become lumpy by cryſtallizing, in which ſtate it could not be ſerved at table. Beſides which, the parts being condenſed by this ſudden cold, are found to reſiſt the action of the air more effectually. With this laſt intention it is alſo proper

Purification of butter by fuſion, which ſeparates the cheeſy part.

to

to cover the pot in which the butter is kept very exactly, and to place it in a cold expoſure, ſuch as a cellar. By this treatment butter may be kept for ſix months or more, and will be nearly as good as freſh butter, particularly after the top is taken off. It is even poſſible to give this fuſed butter to a certain point the appearance of freſh butter, by beating it with one ſixth part of its weight of the cheeſy matter; and ſo likewiſe rancid butter may be conſiderably amended by the proceſs of fuſion and cooling here preſcribed.

SCIENTIFIC NEWS.

Temperature of the Sea.

General facts reſpecting the temperature of the ſea.

MR. PIRON has lately communicated to the French National Inſtitute a memoir on the temperature of the ſea; an intereſting ſubject, capable of being applied to various uſeful purpoſes, and which has accordingly engaged the attention of a conſiderable number of philoſophical obſervers. His general facts are, 1. The mean temperature of the ſea at its ſurface is commonly more elevated than that of the air. 2. It is higher the nearer to the continents and large iſlands. 3. At a diſtance from the ſhore in deep ſeas the water is colder below than at its ſurface; and the more the greater the depth. All the obſervations ſeem to ſhew, that in the abyſſes of the ocean, as well as on the ſummits of mountains, even under the equator, eternal froſt prevails. 4. A ſimilar cold is obſerved in extenſive lakes, and even within the earth at great depths, but it appears to be leſs ſudden. 5. Theſe reſults concur in proving, that the temperature within the earth is not every where the ſame and equal to $93\frac{3}{4}^{\circ}$, as has been long thought (about 50° Fahr. whether this be centigrade or Reaumur's ſcale.)

At great depths it is eternally frozen.

Spent Oil of the Curriers.

Concerning the oil and compounds uſed in currying leather.

The proceſs by which the curriers impregnate their ſkins is is by ſmearing the oil upon the wet ſkin, into which it penetrates as the moiſture evaporates. A pure oil could not perhaps be

be thus ſpread, and moſt probably would not enter the ſkin with the deſired effect, or render it as ſupple as that oil which from experience they are led to prefer.

The celebrated Seguin has directed his attention to this ingredient of ſuch extenſive manufactoring utility. He remarks, that this material (by the name of *Degras)* is of two kinds in France; *viz.* the common ſort and that of Niort. The firſt is the immediate product of the chamoying of ſkins, which are cleared of their ſurplus oil by ſolution of potaſh. It therefore contains not only ſoap, but likewiſe gelatine. It is evaporated to dryneſs and then ſold as *Degras.* At Niort it is decompoſed by ſulphuric acid, and the precipitate is called the *Degras* of that town.

Mr. Seguin finds by analyſis, that this laſt is oxigenated oil, whereas the other is a compound of ſoap and gelatine. He ſucceeded in giving to whale oil all the properties of the *Degras* of Niort, by boiling one pound for a few minutes with half an ounce of nitric acid at 25 degrees. He obſerved that no gas is diſengaged in this operation; but that water and nitrate of ammonia are formed; and he concludes that the oil was oxigenated, not by abſorbing the oxigen of the acid, but by yielding to it part of the hidrogen which was one of its own component parts. The reſult is the more intereſting, as the *Degras* of Niort being much more eſteemed than the common ſort, the curriers may hereafter, inſtead of paying a great price for it, make it in as large quantities as they pleaſe by following the proceſs here indicated.

Note reſpecting the Decompoſition of Sulphate of Lead by the Muriatic Acid. By M. Descotils *.

If the ſulphate of lead be treated with muriatic acid rather concentrated, that metallic ſalt is totally diſſolved, provided the proportion of acid be rather in exceſs. This ſolution requires heat to effect it. Upon cooling, the muriate of lead cryſtallizes in great quantity; and it is much more ſpeedily obtained by the addition of a ſmall quantity of cold water. If the ſupernatant fluid be ſeparated from the cryſtallized ſalt,

Sulphate of lead is ſoluble by heat in muriatic acid: Muriate of lead ſeparates by cooling. This is ſoluble in water, and may be again decompoſed by ſulph. acid.

* Soc. Philom. No. 96.

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a precipitate is obtained from the former by muriate of barytes. The muriate of lead is ſoluble in water, and may be almoſt entirely decompoſed by ſulphuric acid, which forms ſulphate of lead.

Another inſtance in the analyſis of antimonial galena.

This fact deſerves to be carefully examined with relation to the play of affinities; and it may be of importance in the analyſis of mineral and metallic ſubſtances. In fact, if an alloy contain a ſmall quantity of lead, and it were neceſſary, in order to diſſolve the alloy, to employ the nitro-muriatic acid, it would be very poſſible, and I have found it ſo, that ſulphuric acid would not indicate the preſence of lead. The following is another inſtance: If an antimonial galena be treated with nitric acid and ſulphate of lead thus formed, this laſt would be decompoſed by the muriatic oil (*qu.* acid?) which might be employed to take up the oxide of antimony, and the muriate of lead would remain diſſolved after the addition of water. If care were not taken to examine the filtered liquor, a loſs would be experienced which it would be difficult to account for.

Extract of a Letter from Naples, dated Auguſt 13.

Account of the late eruption of Mount Veſuvius.

"Yeſterday at ten o'clock at night, the eruption of Veſuvius, of which the earthquake ſeemed to be the forerunner, took place. We were going to viſit the crater when the cries of the people and a volume of flame informed us that the volcano had opened. The lava precipitated itſelf in three ſeconds from the laſt peak of the mountain and took a direction towards the valley, ſituated between Torre del Greco and Torre del l'Annunziata, two towns on the ſea coaſt, beyond Portici, and ſeven or eight miles from Naples.

We ſet off immediately to ſee this wonderful and tremendous phenomenon nearer. From the place of our departure, we ſaw the whole courſe of the lava, which extended nearly two miles, from the crater to the houſes that join the two towns. The ſight was the moſt magnificently frightful that could be ſeen. I contemplated the caſcades of flame pouring from the top of the mountain, and ſhuddered at ſeeing an immenſe torrent of fire ravage the fineſt fields, overthrow houſes, and deſtroy in a few minutes the hopes and reſources of a hundred families.

A line

Account of the late eruption of Mount Veſuvius.

A line of fire marked the profile of the mountain: a cloud of ſmoke, which ſeemed to ſend forth from time to time flaſhes of lightning, hung over the ſcene, and the moon appeared to be pale: Nothing can adequately deſcribe the grandeur of the ſcene, or give an accurate idea of the horror of it. As we approached the ſpot ravaged by this river of hell, ruined inhabitants having quitted their houſes—deſolated families trying to ſave their furniture and proviſions, laſt and feeble reſource—an immenſe croud of curious ſpectators, retreating ſtep by ſtep from the advancing lava, and teſtifying by extraordinary cries their wonder, fear, and pity—the frightful bellowing of the mountain, the frequent exploſions which burſt from the bottom of the torrent, the crackling of the trees devoured by the flames, the noiſe of the walls falling, and the lugubrious ſound of a bell, which the religious of the Camaldules, iſolated on a little hill and ſurrounded by two torrents of fire, rang in their diſtreſs. Such are the details of the frightful ſcene to which I was witneſs.

The moment we arrived the lava was croſſing the great road below Torre del Greco. To ſee it better we got into a beautiful houſe on the road ſide—from the terrace we ſaw the fire at no more than fifteen paces from us—in a minute we deſcended, and twenty minutes afterwards there remained of the houſe but three large walls. I approached as near as the heat and flow of the current would permit me, I attempted at different times to burn the end of my handkerchief in it—I could only do it by tying it to my cane. The lava does not run in liquid waves; it reſembles an immenſe quantity of coals on fire, which an invincible ſtrength had heaped up and puſhed on with violence. When it met with a wall, it collected to the height of ſeven or ten feet, burnt it, and overthrew it at once. I ſaw ſome walls get red hot, like iron, and melt, if I may uſe the expreſſion, into the lava. In its greateſt ſpeed and on an horizontal road, I reckoned that the torrent travelled at the rate of eighteen inches a minute. Its ſmell reſembled that of iron red hot.

Morning Chronicle.

Applicative

Applicative Compaſs for taking Bearings on a Chart, by N. D. STARCK, *Eſq. of the Royal Navy.*

Compaſs for taking bearings on a chart.

This inſtrument, ſeen in *Fig. 6, Plate* XI. conſiſts of an inner and outer braſs concentric circle; the latter of which, when in uſe, is to be applied to a chart, ſo that its cardinal points may agree with thoſe of the draft, and its central (metallic) point be directly over the ſhip's place. The inner circle is to be ſet to the variation; and the thread from the center being laid, will ſhew either bearings by compaſs, or true bearings, according to the circle upon which they are read. It is obvious alſo, that the inſtrument may be uſed in delineating, plotting, and for various other uſeful purpoſes.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

DECEMBER, 1805.

ARTICLE I.

On the Diviſion of an Arch of a Circle into two ſuch Parts, that their Sines, or Coſines, or Verſed-Sines, ſhall have a given Relation. In a Letter from JOHN GOUGH, *Eſq.*

To Mr. NICHOLSON.

SIR,

BEING at preſent on a viſit to my friend Michael Fryer, teacher of the mathematics at this place, I have availed myſelf of the opportunity to conſult his very extenſive mathematical library, with a view to diſcover how far the following theorems and problems are original; thinking it poſſible, at leaſt, that ſimilar propoſitions might be met with in the works of the early geometricians, particularly in the tracts on Angular Sections, by Vieta, Oughtred, Wallis, and others, which I had never before been able to meet with; but I have found only one of them to have been already treated, of which notice ſhall be taken in its proper place: nevertheleſs, it is not improbable but that ſimilar theorems and problems are ſcattered up and down in the different works on geometry at preſent in exiſtence: As this eſſay, however, may claim the merit of ex- Introductory letter.

exhibiting them in one view, and, which is equally desirable, of deriving them from a general principle, I have ventured to offer it for insertion in your Journal.

JOHN GOUGH.

Reeth, near Richmond, Yorkshire,
August 28.

PROPOSITION I. THEOREM.

Division of an arch of a circle into two parts, having their sines, or cosines, or v. sines, in a given ratio.

Let A F be the arch of a circle, *(See Fig.* 1, *Pl.* XII.) A P a tangent at A; F P a perpendicular to A P, then A P is equal to the sine of A F; F P, the part of the perpendicular intercepted by the tangent and the point F in the arch, is equal to its versed sine; and the same line, P M, intercepted again by the circle in M, is equal to the versed sine of its supplement.

Demonstration.

Draw the diameter A K, and the sine F S perpendicular thereto; also from the center O, draw O L at right angles to P M; then, since P A touches the circle in A, P A K is a right angle, (Euc. 16. iij.) Also, the angles F P A, A S F, are right, by construction; therefore A S F P is a parallelogram, the opposite sides of which are equal, namely, A P = the sine S F, and P F = the versed sine A S, (Euc. 34. i.)

Again, since O L is perpendicular to P M, it is parallel to A P and S F, therefore P L = A O, or O K; and F L = S O, (Euc. 34. I.)—But F L = L M, (Euc. 3. III.) Consequently P M = S K, or the versed sine of the supplement A F. Q. E. D.

PROPOSITION II. THEOREM.

If A F B be an arch of a circle, *(See Fig.* 2.) and A P, B R, be tangents at A and B, from any point, F, in the circumference, draw F P, F R, perpendicular to the two tangents, and F Q also perpendicular to the chord A B, then will the rectangle P F × F R = $\overline{F Q}^2$; and the rectangles A P × B R, and A Q × Q B, will also be equal.

Demonstration.

Join A F, F B, and the triangles P F A, Q F B, are equiangular, because they are right-angled at P and Q, by construction; and the angles P A F, Q B F, are equal, (Euc. 32. III.)

Therefore,

Therefore, as A F : F B : : P F : F Q.

Alſo, the triangles Q F A, R F B, are equiangular, for the ſame reaſons.

Therefore, as A F : F B : : F Q : F R.

Conſequently, as P F : F Q : : F Q : F R, (Euc. 11. V.)

And $PF \times FR = \overline{FQ}^2$, (Euc. 14. VI.)

Q. E. 1° D.

Diviſion of an arch of a circle into two parts, having their ſines, or coſines, or v. ſines, in a given ratio.

Again, by the ſame triangles, as F A : F B : : A P : B Q,

and as F A : F B : : A Q : B R;

hence, as A P : B Q : : A Q : B R,

Whence $AP \times BR = BQ \times AQ$:

Q. E. 2° D.

Corol. 1. Produce the perpendicular F Q till it meets the circumference again in G, and $PA \times RB = FQ \times QG$:

For $PA \times RB = AQ \times QB$ by the propoſition; but $AQ \times QB = FQ \times QG$, (Euc. 35. III.)

Corol. 2. If the lines P F, R F, meet the circle again in M and N, then will $PM \times RN = \overline{QG}^2$:

For $\overline{AP}^2 = FP \times PM$, and $\overline{BR}^2 = FR \times RN$, (by Euc. 36. III.)

Therefore, as $\overline{AP}^2 : FP \times PM :: FR \times RN : \overline{BR}^2$:

But $\overline{AP}^2 : FQ \times QG :: FQ \times QG : \overline{BR}^2$, by Corol. 1.

And P F : F Q : : F Q : F R, by the propoſition.

Therefore,

$PF \times PM : FQ \times PM :: FQ \times RN : FR \times RN$.

Hence,

$FQ \times QG : FQ \times PM :: FQ \times RN : FQ \times QG$.

Conſequently, $PM \times RN = \overline{QG}^2$.

Corol. 3. Draw the diameters A K, B D, and make F S, F T perpendicular to A K, B D; then $AK \times BT$ (the rectangle of the verſed ſines) $= \overline{FQ}^2$; $SF \times FT$ (the rectangle of the ſines) $= AQ \times QB$; and $SK \times TD$ (the rectangle of the ſupplementary verſed ſines) $= \overline{QG}^2$. Theſe things follow from Props. I. and II.

Proposition III. Problem.

To divide a given arch of a circle (A B) into two parts (A F, F B), ſo that the rectangle of their verſed ſines (A S, B T) may be equal to a given magnitude, or ſquare, $(m \times m)$.

Conſtruction.

Diviſion of an arch of a circle into two parts, having their ſines, or coſines, or v. ſines, in a given ratio.

From any point, Y, in the right line A B, draw Y W at right angles to the ſame, making it equal to the given right line m; through W, parallel to Y B, draw W F, and let it cut the arch A B in F, then will A F, F B, be the required arches.

Demonſtration.

Draw F Q perpendicular to Y B, then $F Q^2 = W Y^2 = m \times m$; by Conſt. and Euc. 34. I.; but the rectangle of the verſed ſines of A F and F B $= \overline{F Q}^2$, (by Cor. 3. Prop. II.); therefore this rectangle is equal to $m \times m$, the given ſquare. Q. E. D.

PROPOSITION IV. PROBLEM.

To divide A F B, a given arch of a circle, *(See Fig. 3.)* into two parts, A F, F B, ſo that the rectangle of their ſines may be equal to a given ſquare, $(n \times n)$?

Conſtruction.

To make the conſtruction general, let A F B be greater than a ſemicircle, join A B, and in it take Q, making A Q $\times$ Q B $= n \times n$; alſo in A B produced take q, ſo as to make A $q \times q$ B $= n \times n$; draw Q F, $q f g$, perpendicular to A B; then will A F, F B, or A g, g B, or A f, f B, be the required arches.

Demonſtration.

This is evident from Cor. 3. Prop. II. and the conſtruction.

To find *the limits*, biſect A B in Z, draw alſo the radius O N parallel to Z B, and make N E perpendicular to A B produced; then, if $n \times n$ be greater than A Z $\times$ Z B, F is an imaginary point, becauſe A Q $\times$ Q B cannot exceed A Z $\times$ Z B, by Euc. 5. II. Again, if $n \times n$ be greater than A E $\times$ E B, the points f, g, are imaginary, becauſe A $q \times q$ B cannot exceed A E $\times$ E B, ſeeing E N touches the circle in N, and is parallel to $q g$: Theſe things being premiſed, it will be eaſily perceived, that when A F B is leſs than a ſemicircle, it can only be divided in one point to anſwer the conditions of the queſtion, becauſe the point N will be in the oppoſite ſegment; but when it exceeds a ſemicircle, it will admit of being divided into one, two, or three points, according to circumſtances, or even the conſtruction may prove impoſſible. Q. E. D.

Scholium

Scholium.

This problem is conſtructed at page 342 of the Appendix to Simpſon's Algebra, 2d Edition; and at page 140 of his Select Exerciſes, 1ſt Edition; but the conſtructions given by that able geometrician do not ſhew the various limits of the queſtion with that degree of perſpicuity which appears in the preſent method.

Diviſion of an arch of a circle into two parts, having their ſines, or coſines or v. ſines, in a given ratio.

Lemma.

Let A B C D be a ſquare, *(See Fig.* 4.) from any two adjacent ſides of which, C B, C D, take the ſegments T C, C S, then will the rectangle of the remaining ſegments B T $\times$ S D $= \overline{BC}^2 + TC \times CS - BC \times CT - BC \times CS$.

Demonſtration.

Draw S G, T H, parallel to B C, C D, and let them interſect in F;—

Then the rectangle F T C S $=$ T C $\times$ C S,

and the rectangle F H A G $=$ B T $\times$ S D,

But F H A G $+$ G B C S $+$ H F S D $=$ the ſquare A B C D; (Euc. 1. II.)

Add F T C S to both,

Then F H A G $+$ G B C S $+$ T C D H $=$ A B C D $+$ F T C S;

But C D is equal to B C,

Therefore F H A G $=$ A B C D $+$ F T C S $-$ B C $\times$ C T $-$ B C $\times$ C S;

That is, B T $\times$ S D $= \overline{BC}^2 + TC \times CS - BC \times CT -$ B C $\times$ C S. Q. E. D.

PROPOSITION V. PROBLEM.

To divide A F B, a given arch of a circle, *(See Fig.* 5.) into two parts, A F, and F B; ſo that the rectangle of their coſines may be equal to a given ſquare, $k \times k$?

Conſtruction.

Join A B, and from the center, O, draw O Z perpendicular to A B; in Z O take Z V equal to the given line, k, and join B V; draw the diameter, H h, parallel to A B, and divide it in I ſo as to make H I $\times$ I $h = \overline{BV}^2$; from I draw I Q perpendicular to A B, and when produced let it meet the given arch in F; then will A F, F B, be the required arches.

Demonſtration.

Demonſtration.

Diviſion of an arch of a circle into two parts, having their ſines, or coſines, or v. ſines, in a given ratio.

Let F I meet the circle again in G, draw the diameters A K, B D, and the ſines F S, F T;

Then the coſine O S = O A − A S,

and the coſine O T = O B − B T = O A − B T;

Hence $S O \times O T = \overline{A O}^2 + A S \times B T - A O \times A S - A O \times B T$;

But $A S \times B T = \overline{F Q}^2$, by Prop. II.

Therefore,

$S O \times O T = \overline{A O}^2 + \overline{F Q}^2 - A O \times A S - A O \times B T$.

Again, K S = 2 A O − A S,

and D T = 2 A O − B T;

Hence, $K S \times D T = 4\,\overline{A O}^2 + A S \times B T - 2 A O \times A S - 2 A O \times B T$;

But $K S \times D T = \overline{Q G}^2$, by Cor. 3. Prop. II.

Therefore,

$4\,\overline{A O}^2 + \overline{F Q}^2 - 2 A O \times A S - 2 A O \times B T = \overline{Q G}^2$;

Hence, $A O \times A S + A O \times B T = \overline{2 A O}^2 + \frac{\overline{F Q}^2}{2} - \frac{\overline{Q G}^2}{2}$:

But $S O \times O T = A O^2 + F Q^2 - A O \times A S - A O \times B T$

$= \frac{F Q^2}{2} + \frac{Q G^2}{2} - \overline{A O}^2$.

F Q = F I − I Q = B P − O Z,

and G Q = G I + I Q = B V + O Z;

Hence $\frac{F Q^2}{2} + \frac{G Q^2}{2} = \overline{B V}^2 + \overline{O Z}^2$;

Conſequently, $S O \times O T = \overline{B V}^2 + \overline{O Z}^2 - \overline{A O}^2$;

But $\overline{A O}^2 - \overline{O Z}^2 = \overline{B O}^2 - \overline{O Z}^2 = \overline{B Z}^2$;

Therefore $S O \times O T = \overline{B V}^2 - \overline{B Z}^2 = \overline{V Z}^2$,

(Euc. 47. I.) $= k \times k$, by conſtruction. Q. E. D.

Limitation.—If Z V be greater than Z O, B V will be greater than B O; *i. e.* F G will be greater than H *h*, which is impoſſible, Euc. 15. III. therefore Z V, *or k*, cannot exceed Z O.

Proposition VI. Problem.

To divide A F B, a given arch of a circle, *(See Fig.* 6.) into two parts, ſo that the ſum of their verſed ſines may be equal to a given right line, *u?*

Conſtruction.

Conſtruction.

Division of an arch of a circle into two parts, having their ſines, or coſines, or v. ſines, in a given ratio.

Draw the radius A O, and the tangent B E; in A O take A I equal to the given line, *u*; and making I V perpendicular to A O, let it meet B E in V; draw F V to biſect the angle E V I, and let it cut the given arch in F; then will A F, F B, be the required arches.

Demonſtration.

Draw the tangent A G, which is parallel to I V, alſo make F P, F R perpendicular to A G, B E, and let P F produced meet I V in H.

Then ſince A G, I V, are parallels, and the angle H P A is right, F H V is alſo a right angle, (by Euc. 29. I.) therefore it is equal to the angle F R V, (by conſtruction.)

But the angles R V F, H V F, are alſo equal, (by conſtruction); conſequently the triangles R F V, H F V, are equiangular; and they have one ſide common, namely the ſide V F; therefore F R = F H, (Euc. 4. VI.) and P F + F R = P H = A I, (Euc. 34. I.) = *u*, (by conſtruction.)

But the ſum of the verſed ſines of A F, F B, is equal to P F + F R, (by Prop. I.) therefore this ſum is equal to the given line, *u*. Q. E. D.

Limitation.—If A I be greater than the verſed ſine of the whole arch A B, the point F will evidently fall in the oppoſite ſegment, and the conſtruction will be impoſſible.

Again, ſince the angle I V E is equal to the angle A O B, draw the radius O C, to biſect the angle A O B, and it will evidently be perpendicular to V F; therefore L C, a tangent at C, will be parallel to V F; conſequently if A I be ſo taken, that V may lie in B L produced, the conſtruction will alſo be impoſſible; which will therefore happen when *u* is leſs than twice the verſed ſine of the arch A C, or B C.

Corol. Since the ſum of the verſed ſines of two arches is the ſame with the difference of the diameter and the ſum of the coſines, if the latter ſum be given the problem may be conſtructed by the laſt propoſition.

PROPOSITION VII. PROBLEM.

To divide A F B, a given arch of a circle, (*Fig.* 7.) into two parts, ſo that the ſum of their ſines may be equal to a given right line, *w*?

Conſtruction.

Conſtruction.

Diviſion of an arch of a circle into two parts, having their ſines, or coſines, or v. ſines, in a given ratio.

Draw the radii A O, O B, and the tangents A G, B E, in which take A S, B T, each equal to the half of w; draw S N, T N, parallel to A O, O B; and through their interſection, N, draw N F, parallel to S T, to meet the arch in F, then A F, F B, are the parts required.

Demonſtration.

Draw F K, F M, parallel to N S, N T, and let them meet S T in K, M, and A G, B E, in P, R; then it is eaſily proved that the triangles K F M, S N T, are equal and ſimilar, and that K M $=$ S T; conſequently S K $=$ T M.

But the angles K P S, M R T, are right, being equal to the angles O A G, O B E, by conſtruction; and the angles K S P, M T R, are equal; therefore the triangles P S K, R T M, are equiangular, they are therefore equal, (Euc. 4. VI.) becauſe S K $=$ T M; conſequently S P $=$ R T; therefore A P $+$ B R $=$ A S $+$ B T $=$ w.

But the ſum of the ſines of A F, F B $=$ A P $+$ B R; this ſum is therefore equal to w.

Limitation.—Join A B, which will be parallel to S F, alſo let the radius O C biſect the angle A O B, when properly produced, or not, it will paſs through the point N. Now if N be in O C produced, N F, being parallel to S T, or A B, will not meet the circle; on the other hand, if N lie between O and A B, F will be in the oppoſite ſegment of the circle, conſequently the conſtruction is impoſſible, unleſs N fall between C and the line A B, or in the verſed ſine of half the given arch: Theſe things being premiſed, it will be eaſily perceived that the ſine of the arch A F B is the leſs limit of the problem, and twice the ſine of A C its greater limit.

Concerning

II.

Concerning the State in which the true Sap of Trees is depoſited during Winter. By THOMAS ANDREW KNIGHT, *Eſq* *.

IT is well known that the fluid, generally called the ſap in trees, aſcends in the ſpring and ſummer from their roots, and that in the autumn and winter it is not, in any conſiderable quantity, found in them; and I have obſerved in a former paper, that this fluid riſes wholly through the alburnum, or ſap-wood. But Du Hamel and ſubſequent naturaliſts have proved, that trees contain another kind of ſap, which they have called the true, or peculiar juice, or ſap of the plant. Whence this fluid originates does not appear to have been agreed by naturaliſts; but I have offered ſome facts to prove that it is generated by the leaf †; and that it differs from the common aqueous ſap owing to changes it has undergone in its circulation through that organ: and I have contended that from this fluid (which Du Hamel has called the *ſuc propre*, and which I will call the true ſap,) the whole ſubſtance, which is annually added to the tree, is derived. I ſhall endeavour in the preſent paper to prove that this fluid, in an inſpiſſated ſtate, or ſome concrete matter depoſited by it, exiſts during the winter in the alburnum, and that from this fluid, or ſubſtance, diſſolved in the aſcending aqueous ſap, is derived the matter which enters into the compoſition of the new leaves in the ſpring, and thus furniſhes thoſe organs, which were not wanted during the winter, but which are eſſential to the further progreſs of vegetation.

The common ſap riſes in ſpring and ſummer, but not in winter.

True or peculiar ſap exiſts in the alburnum during winter: Its ſubſequent deſtination pointed out.

Few perſons at all converſant with timber are ignorant, that the alburnum, or ſap-wood of trees, which are felled in the autumn or winter, is much ſuperior in quality to that of other trees of the ſame ſpecies, which are ſuffered to ſtand till the ſpring, or ſummer: it is at once more firm and tenacious in its texture, and more durable. This ſuperiority in winter-felled wood has been generally attributed to the abſence of the ſap at that ſeaſon; but the appearance and qualities of the

That winter or autumn felled wood has its alburnum more firm, &c. commonly attributed to the abſence of the ſap:

* See Phil. Tranſ. of 1801, page 336.

† Philoſ. Tranſ. 1805, p. 88.

wood

wood ſeem more juſtly to warrant the concluſion, that ſome ſubſtance has been added to, inſtead of taken from it, and many circumſtances induced me to ſuſpect that this ſubſtance is generated, and depoſited within it, in the preceding ſummer and autumn.

—but probably to its preſence.

Du Hamel has remarked, and is evidently puzzled with the circumſtance, that trees perſpire more in the month of Auguſt, when the leaves are full grown, and when the annual ſhoots have ceaſed to elongate, than at any earlier period; and we cannot ſuppoſe the powers of vegetation to be thus actively employed, but in the execution of ſome very important operation. Bulbous and tuberous roots are almoſt wholly generated after the leaves and ſtems of the plants, to which they belong, have attained their full growth; and I have conſtantly found, in my practice as a farmer, that the produce of my meadows has been immenſely increaſed when the herbage of the preceding year had remained to perform its proper office till the end of the autumn, on ground which had been mowed early in the ſummer. Whence I have been led to imagine, that the leaves, both of trees and herbaceous plants, are alike employed, during the latter part of the ſummer, in the preparation of matter calculated to afford food to the expanding buds and bloſſoms of the ſucceeding ſpring, and to enter into the compoſition of new organs of aſſimilation.

Full grown leaves perſpire moſt plentifully,

—and at this period the vegetative powers appear to be employed in increaſing the growth of the vegetable.

If the preceding hypotheſis be well founded, we may expect to find that ſome change will gradually take place in the qualities of the aqueous ſap of trees during its aſcent in the ſpring; and that any given portion of winter-felled wood will at the ſame time poſſeſs a greater degree of ſpecific gravity, and yield a larger quantity of extractive matter, than the ſame quantity of wood which has been felled in the ſpring or in the early part of the ſummer. To aſcertain theſe points I made the experiments, an account of which I have now the honour to lay before you.

If this be the caſe, it ſhould be found that the aqueous ſap muſt be altered in its aſcent; and the winter felled wood will be denſer.

As early in the laſt ſpring as the ſap had riſen in the ſycamore and birch, I made inciſions into the trunks of thoſe trees, ſome cloſe to the ground, and others at the elevation of ſeven feet, and I readily obtained from each inciſion as much ſap as I wanted. Aſcertaining the ſpecific gravity of the ſap of each tree, obtained at the different elevations, I found that of the ſap of the ſycamore with very little variation, in dif-

Experiments. Birch and ſycamore in ſpring gave ſap moſt aqueous near the bottom; but denſer and more ſaccharine the higher up.

ferent

ferent trees, to be 1.004 when extracted close to the ground, and 1.008 at the height of seven feet. The sap of the birch was somewhat lighter; but the increase of its specific gravity, at greater elevation, was comparatively the same. When extracted near the ground the sap of both kinds was almost free from taste; but when obtained at a greater height, it was sensibly sweet. The shortness of the trunks of the sycamore trees, which were the subjects of my experiments, did not permit me to extract the sap at a greater elevation than seven feet, except in one instance, and in that, at twelve feet from the ground, I obtained a very sweet fluid, whose specific gravity was 1.012.

The sap first drawn was densest; which shews that its augmentation was had from matter in the alburnum.

I conceived it probable, that if the sap in the preceding cases derived any considerable portion of its increased specific gravity from matter previously existing in the alburnum, I should find some diminution of its weight, when it had continued to flow some days from the same incision, because the alburnum in the vicinity of that incision would, under such circumstances, have become in some degree exhausted: and on comparing the specific gravity of the sap which had flowed from a recent and an old incision, I found that from the old to be reduced to 1.002, and that from the recent one to remain 1.004, as in the preceding cases, the incision being made close to the ground. Wherever extracted, whether close to the ground, or at some distance from it, the sap always appeared to contain a large portion of air.

It is difficult to make experiments on the density or sp. gravity of the alburnum.

In the experiments to discover the variation in the specific gravity of the alburnum of trees at different seasons, some obstacles to the attainment of any very accurate results presented themselves. The wood of different trees of the same species, and growing in the same soil, or that taken from different parts of the same tree, possesses different degrees of solidity; and the weight of every part of the alburnum appears to increase with its age, the external layers being the lightest. The solidity of wood varies also with the greater or less rapidity of its growth. These sources of error might apparently have been avoided by cutting off, at different seasons, portions of the same trunk or branch: but the wound thus made might, in some degree, have impeded the due progress of the sap in its ascent, and the part below might have been made heavier by the stagnation of the sap, and that above

lighter

Method adopted. By felling poles in an oak coppice in winter and ſpring and comparing them.

lighter by privation of its proper quantity of nutriment. The moſt eligible method therefore, which occurred to me, was to ſelect and mark in the winter ſome of the poles of an oak coppice, where all are of equal age, and where many, of the ſame ſize and growing with equal vigour, ſpring from the ſame ſtool. One half of the poles which I marked and numbered were cut on the 31ſt of December, 1803, and the remainder on the 15th of the following May, when the leaves were nearly half grown. Proper marks were put to diſtinguiſh the winter-felled from the ſummer-felled poles, the bark being left on all, and all being placed in the ſame ſituation to dry.

The winter felled wood was denſeſt after ſeaſoning,

In the beginning of Auguſt I cut off nearly equal portions from a winter and ſummer-felled pole, which had both grown on the ſame ſtool; and both portions were then put in a ſituation, where, during the ſeven ſucceeding weeks, they were kept very warm by a fire. The ſummer-felled wood was, when put to dry, the moſt heavy; but it evidently contained much more water than the other, and, partly at leaſt, from this cauſe, it contracted much more in drying. In the beginning of October both kinds appeared to be perfectly dry, and I then aſcertained the ſpecific gravity of the winter-felled wood to be 0.679, and that of the ſummer-felled wood to be 0.609; after each had been immerſed five minutes in water.

—by more than ten per cent.

This difference of ten *per cent.* was conſiderably more than I had anticipated, and it was not till I had ſuſpended and taken off from the balance each portion, at leaſt ten times, that I ceaſed to believe that ſome error had occurred in the experiment: and indeed I was not at laſt ſatisfied till I had aſcertained by means of compaſſes adapted to the meaſurement of ſolids, that the winter-felled pieces of wood were much leſs than the others which they equalled in weight.

The difference was not quite ſo much in the newly formed layers of each.

The pieces of wood, which had been the ſubjects of theſe experiments, were again put to dry, with other pieces of the ſame poles, and I yeſterday aſcertained the ſpecific gravity of both with ſcarcely any variation in the reſult. But when I omitted the medulla, and parts adjacent to it, and uſed the layers of wood which had been more recently formed, I found the ſpecific gravity of the winter-felled wood to be only 0.583, and that of the ſummer-felled to be 0.533; and trying the ſame experiment with ſimilar pieces of wood, but taken from

poles

poles which had grown on a different ſtool, the ſpecific gravity of the winter-felled wood was 0.588, and that of the ſummer-felled 0.534.

The winter felled wood gave out a larger portion of extract.

It is evident that the whole of the preceding difference in the ſpecific gravity of the winter and ſummer-felled wood might have ariſen from a greater degree of contraction in the former kind, whilſt drying; I therefore proceeded to aſcertain whether any given portion of it, by weight, would afford a greater quantity of extractive matter, when ſteeped in water. Having therefore reduced to ſmall fragments 1000 grains of each kind, I poured on each portion ſix ounces of boiling water; and at the end of twenty-four hours, when the temperature of the water had ſunk to 60°, I found that the winter-felled wood had communicated a much deeper colour to the water in which it had been infuſed, and had raiſed its ſpecific gravity to 1.002. The ſpecific gravity of the water in which the ſummer-felled wood had, in the ſame manner, been infuſed was 1.001. The wood in all the preceding caſes was taken from the upper parts of the poles, about eight feet from the ground.

Probability that this ſap is exhauſted more or leſs by the leaves and ſhoots.

Having obſerved, in the preceding experiments, that the ſap of the ſycamore became ſpecifically lighter when it had continued to flow during ſeveral days from the ſame inciſion, I concluded that the alburnum in the vicinity of ſuch inciſion had been deprived of a larger portion of its concrete or inſpiſſated ſap than in other parts of the ſame tree: and I therefore ſuſpected that I ſhould find ſimilar effects to have been produced by the young annual ſhoots and leaves; and that any given weight of the alburnum in their vicinity would be found to contain leſs extractive matter than an equal portion taken from the lower parts of the ſame pole, where no annual ſhoots or leaves had been produced.

Experiment ſhewed that they leave a certain portion of extract in the trunk.

No information could in this caſe be derived from the difference in the ſpecific gravity of the wood; becauſe the ſubſtance of every tree is moſt denſe and ſolid in the lower parts of its trunk; and I could on this account judge only from the quantity of extractive matter which equal portions of the two kinds of wood would afford. Having therefore reduced to pieces ſeveral equal portions of wood taken from different parts of the ſame poles, which had been felled in May, I poured on each portion an equal quantity of boiling water, which I ſuffered

ſuffered to remain twenty hours, as in the preceding experiments: and I then found that in ſome inſtances the wood from the lower, and in the others that from the upper parts of the poles, had given to the water the deepeſt colour and greateſt degree of ſpecific gravity; but that all had afforded much extractive matter, though in every inſtance the quantity yielded was much leſs than I had, in all caſes, found in ſimilar infuſions of winter-felled wood.

Hence many trees have a ſucceſſion of leaves and buds.

It appears, therefore, that the reſervoir of matter depoſited in the alburnum is not wholly exhauſted in the ſucceeding ſpring: and hence we are able to account for the ſeveral ſucceſſions of leaves and buds which trees are capable of producing when thoſe previouſly protruded have been deſtroyed by inſects, or other cauſes; and for the extremely luxuriant ſhoots, which often ſpring from the trunks of trees, whoſe branches have been long in a ſtate of decay.

The matter in the alburnum may remain inactive for ſeveral years.

I have alſo ſome reaſons to believe that the matter depoſited in the alburnum remains unemployed in ſome caſes during ſeveral ſucceſſive years: it does not appear probable that it can be all employed by trees which, after having been tranſplanted, produce very few leaves, or by thoſe which produce neither bloſſoms nor fruit. In making experiments in 1802, to aſcertain the manner in which the buds of trees are reproduced, I cut off in the winter all the branches of a very large old pear tree, at a ſmall diſtance from the trunk; and I pared off, at the ſame time, the whole of the lifeleſs external bark.

Inſtance: in an old pear tree.

The age of this tree, I have good reaſon to believe, ſomewhat exceeded two centuries: its extremities were generally dead; and it afforded few leaves, and no fruit; and I had long expected every ſucceſſive year to terminate its exiſtence. After being deprived of its external bark, and of all its buds, no marks of vegetation appeared in the ſucceeding ſpring, or early part of the ſummer; but in the beginning of July numerous buds penetrated through the bark in every part, many leaves of large ſize every where appeared, and in the autumn every part was covered with very vigorous ſhoots, exceeding, in the aggregate, two feet in length. The number of leaves which, in this caſe, ſprang at once from the trunk and branches appeared to me greatly to exceed the whole of thoſe which the tree had born in the three preceding ſeaſons; and I

cannot believe that the matter which compoſed theſe buds and leaves could have been wholly prepared by the feeble vegetation and ſcanty foliage of the preceding year.

But whether the ſubſtance which is found in the alburnum of winter-felled trees, and which diſappears in part in the ſpring and early part of the ſummer, be generated in one or in ſeveral preceding years, there ſeem to be ſtrong grounds of probability, that this ſubſtance enters into the compoſition of the leaf: for we have abundant reaſon to believe that this organ is the principal agent of aſſimilation; and ſcarcely any thing can be more contrary to every concluſion we ſhould draw from analogical reaſoning and compariſon of the vegetable with the animal economy, or in itſelf more improbable, than that the leaf, or any other organ, ſhould ſingly prepare and aſſimilate immediately from the crude aqueous ſap, that matter which compoſes itſelf.

It is ſtrongly probable that this matter compoſes the leaf.

It has been contended * that the buds themſelves contain the nutriment neceſſary for the minute unfolding leaves; but trees poſſeſs a power to reproduce their buds, and the matter neceſſary to form theſe buds muſt evidently be derived from ſome other ſource: nor does it appear probable that the young leaves very ſoon enter on this office: for the experiments of Ingenhouz prove that their action on the air which ſurrounds them is very eſſentially different from that of full grown leaves. It is true that buds in many inſtances will vegetate, and produce trees, when a very ſmall portion only of alburnum remains attached to them; but the firſt efforts of vegetation in ſuch buds are much more feeble than in others to which a larger quantity of alburnum is attached, and therefore we have, in this caſe, no grounds to ſuppoſe that the leaves derive their firſt nutriment from the crude ſap.

It is not likely that they are ſupported by the crude ſap.

It is alſo generally admitted, from the experiments of Bonnet and Du Hamel, which I have repeated with the ſame reſult, that in the cotyledons of the ſeed is depoſited a quantity of nutriment for the bud, which every ſeed contains; and though no veſſels can be traced † which lead immediately from the cotyledons to the bud or plumula, it is not difficult to point out a more circuitous paſſage, which is perfectly ſimilar to that through which I conceive the ſap to be carried from the

Seeds are thus nouriſhed not from the ſoil, but from matter depoſited in the cotyledons.

* Thomſon's Chemiſtry. † Hedwig.

leaves

leaves to the buds, in the ſubſequent growth of the tree; and I am in poſſeſſion of many facts to prove that ſeedling trees, in the firſt ſtage of their exiſtence, depend entirely on the nutriment afforded by the cotyledons; and that they are greatly injured, and in many inſtances killed, by being put to vegetate in rich mould.

(To be concluded in the Supplement.)

III.

On the Deliqueſcence and Efflores ence of Salts. By C. L. CADET.*

Deliqueſcence and efflores ence are occaſioned by the relative attractions of the air and of each ſalt for water.

ALL chemiſts are of the ſame opinion relative to the cauſe of the deliqueſcence or the efflores ence of a ſalt. The attraction of the ſalt for the water contained in the atmoſphere occaſions the firſt phenomenon, the attraction of the atmoſpheric air for the water of cryſtallization of the ſalt cauſes the ſecond.

The difference between one ſalt and another has been noticed, but not the variations from the ſtate of the air, &c.

This attraction has been found to vary in the different ſalts, whether deliqueſcent or efflores ent, to be ſtronger in ſome kinds, and more ſpeedy in others; but no one has yet obſerved whether it had any dependence on the conſtitution of the atmoſphere, the electric ſtate of the air, the quantity of caloric it contained, if it was always the ſame in any one ſalt, and if it regularly became weaker in proportion as ſaturation approached, neither have any tables been yet prepared, which might indicate the degree of deliqueſcence, or of efflores ence of the different ſalts.

Of the hypotheſes which could be made on theſe phenomena, the following ſeemed moſt probable.

Hypotheſes that deliqueſcent ſalts ſhould attract water in proportion as the hygrometer indicated its preſence.

The ſalts which deprived the air of its humidity ought to act in this reſpect in proportion to the quantity of water which the air held in ſolution or in ſuſpenſion. The greater the humidity of the air, the more ſhould the deliqueſcent ſalts augment in weight, ſo that the degree of their weight ſhould be conformable to the progreſs of the hygrometer.

Barometrical changes and

On the other hand atmoſpheric preſſure, which more or leſs oppoſes evaporation, ought to have an influence on the ſatura-

* Journal de Phyſique, LX.

tion

tion of the ſalts, ſince it cauſes the denſity of the air to vary; conſequently there ſhould be an agreement between the variations of the barometer and the deliqueſcence of ſalts.

The variations of temperatures, by dilating, or by condenſing the maſs of the atmoſphere, ſhould alſo occaſion changes in the proportion of water abſorbed by the ſalts, on which account it would be uſeful to obſerve the thermometer.

—thermometrical ſhould alſo have their influence.

I thought moreover that one ſalt had not only more or leſs attraction for the water contained in the air than another, but that this attraction varied likewiſe in the ſame ſalt in proportion as it had loſt or abſorbed water. I hoped by thus comparing the deliqueſcence and efflureſcence of ſalts with the ſtate of the different meteorological inſtruments, to obtain reſults ſufficiently conſtant to eſtabliſh a theory of deliqueſcence or efflureſcence. I hoped alſo to be able to uſe the ſalts themſelves as inſtruments of meteorological obſervation; but experience proved that reaſoning apparently founded on the trueſt theory frequently deceives expectation. It is nevertheleſs neceſſary to attend to negative facts, which ſometimes are as ſerviceable to ſcience as thoſe of a poſitive nature.

Deliqueſcent ſalts ſhould attract moſt when leaſt ſaturated.

Experience did not confirm theſe poſitions.

I did not find a ſingle ſalt which ſeemed to have the leaſt conformity with the ſtate of the barometer, hygrometer, or thermometer. On the ſame day many ſalts increaſed conſiderably in weight, while others indicated a ſlow progreſs. Some had but a ſmall attraction, when the hygrometer ſhewed a great degree of humidity, and were moſt deliqueſcent when the air ſeemed moſt dry. Atmoſpheric preſſure never had the leaſt agreement with the increaſe of weight of a ſalt, and the thermometer having varied but half a degree during the courſe of the experiments, does not furniſh any obſervation on the influence of temperature. It is therefore impoſſible to explain by the meteorological changes any of the variations which I obſerved in the deliqueſcence, or the efflureſcence of ſalts.

None of the ſalts appeared to gain or loſe weight in conformity to meteoric changes.

Efflureſcent Salts.

Experiments. Efflureſcent ſalts, ſulphate, phoſphate and carbonate of ſoda expoſed.

I weighed exactly 288 grains of ſulphate of ſoda, of phoſphate of ſoda, and of carbonate of ſoda, which three ſalts are conſidered as the moſt efflureſcent, and placed them in a dry and airy ſituation, after having carefully dried the capſules which contained them. I put alſo in the ſame place an hygrometer, a barometer, and a thermometer: the three ſalts ſhewed the following reſults.

Loſs of weight in each by efflorescence.

	Left to efflorescence.	Loſt.
Sulphate of ſoda	61 days	203 grains of water.
Phoſphate of ſoda	39	91
Carbonate of ſoda	51	86

It ſhould ſeem from this table that theſe three ſalts ought to be claſſed in the preceding order; but it muſt be obſerved that ſalts contain more or leſs water, in proportion as they cryſtalize ſlowly or rapidly. The number of days which were employed in the efflorescence of theſe ſalts ſhould vary, both in proportion to the water they contained, and to the extent of ſurface which they expoſed to the action of the ſurrounding air; and therefore the time of their efflorescence can give no appreciation of the force of their attraction for water. This reflection prevented my making experiments on any more efflorescent ſalts.

Conſiderations why the time ſeems to indicate no uſeful reſult in deliqueſcent ſalts expoſed.

Deliqueſcent Salts.

I took 288 grains of each of the ſalts in the following table, (which are very ſenſibly deliqueſcent, ſince they all abſorbed more than half their weight of water), and placed each of them in a dried capſule, along with the before-mentioned meteorological inſtruments, in a damp ſituation, and after 150 days of obſervations noted what is included in the table.

A Table of Deliqueſcent Salts, in the Order of their Attraction, eſtimated by the Quantity of Water abſorbed.

Table of the increaſe of weight in each of 19 different ſpecies, and the times reſpectively.

	Days employed in their ſaturation.	Water abſorbed.
Acetite of potaſh	146	700 grains.
Muriate of lime	124	684
Muriate of manganeſe	105	629
Nitrate of manganeſe	89	527
Nitrate of zinc	124	495
Nitrate of lime	147	448
Muriate of magneſia	139	441
Nitrate of copper	128	397
Muriate of antimony	124	388
Muriate of alumine	149	342
Nitrate of alumine	147	300
Muriate of zinc	76	294
Nitrate of ſoda	137	257
Nitrate of magneſia	73	207

	Days employed in their ſaturation.	Water abſorbed.
Acetite of alumine - -	104 -	202
Acid ſulphate of alumine - -	121 -	202
Muriate of biſmuth - -	114 -	174
Acid phoſphate of lime - -	93 -	155
Muriate of copper - - -	119 -	148

In examining this table it may be remarked that the duration of the abſorption is not in any proportion to the quantity: The muriate of alumine, for example, took 149 days to abſorb 342 grains of water, while the nitrate of manganeſe took but 89 days to abſorb 527 grains. That the force of attraction may be eſtimated from the rapidity with which the bodies unite muſt not be concluded from this; for the ſame table ſhews that nitrate of magneſia ſaturated itſelf in 73 days, and only abſorbed 207 grains of water, a much leſs quantity than that taken up by the nitrate of manganeſe. Although the greater or leſs facility with which deliqueſcent ſalts ſaturate themſelves with water cannot be accounted for, (ſince a ſalt half ſaturated, or half deprived of water, is no longer the ſame body, and conſequently exerciſes other attractions than what the ſame ſalt does in its ordinary ſtate, or in a different ſtate of ſaturation,) the rapidity of their ſaturation is not however an indifferent matter. In the experiments which have been made on producing artificial cold by muriate of lime, it has been remarked that the cold was greater in proportion as the ice was melted; but it is probable that the muriate, and above all the nitrate of manganeſe, which becomes liquid much quicker, would produce with ice a more intenſe cold, and that certain liquors which have hitherto reſiſted coagulation, would be ſolidified by theſe two ſalts, which experiment is highly deſerving of a trial.

The times of abſorption were not at all proportional to the quantities.

Though the rapidity of abſorption does not indicate the proportional affinity yet the facts are uſeful.

For inſtance, the ſalts of manganeſe may produce intenſe refrigeration.

In order to examine whether deliqueſcence depends on the proportion of the baſe, or of the acid which conſtitutes the ſalts, I compared with each other the different analyſes of ſalts publiſhed by Bergman, Klaproth, Fourcroy, and Vauquelin, and I ſaw that no induction could be from their compoſition; for there are ſome ſalts which have the baſe in a very conſiderable proportion, and which are leſs deliqueſcent than thoſe whoſe baſe is leſs; and many others in which the acid is in a ſmall proportion, are more deliqueſcent than thoſe, in which this principle is predominant. The nature of the acids and of

It does not appear that the deliqueſcence depends on the proportion of component parts,

—nor on the peculiar nature of the ingredients themſelves.

the baſes themſelves do not throw more light on the phenomena of deliqueſcence than their proportions; for there are deliqueſcent ſalts, the component parts of which taken ſeparately, have not any remarkable attraction for water, ſuch is the nitrate of alumine; while on the other hand the ſulphate of ſoda is efflureſcent, although concentrated ſulphuric acid, and cauſtic ſoda each ſeparately attract humidity. Nothing better proves this axiom in chemiſtry,—*Compounds have properties peculiar to themſelves, and differing from thoſe of their component parts.*

Generally the deliqueſcence was moſt rapid when the ſaturation was leaſt.

In general deliqueſcent ſalts encreaſe their weight in a diminiſhing proportion, according as they approach ſaturation; thus the acetate of potaſh, which in the firſt twenty days exhibited the following progreſſion: 21. 34. 44. 54. 60. 70. 85. 100. 110. 120. 128. 138. 142. 148. 160. 169. 177. 186. 192. 198. did not ſhew on the laſt twenty days more than this, 647. 650. 655. 660. 663. 666. 669. 671. 676. 682. 684. 686. 688. 690. 692. 694. 696. 698. 699. 670. The ſalts which were but little deliqueſcent preſented a ſingular phenomenon, which none, I believe, has obſerved before.

Remarkable facts; ſalts which loſe part of the abſorbed water and afterwards attract more and encreaſe till ſaturation.

The acid ſulphate of alumine, and the acid phoſphate of lime, increaſed and diminiſhed ſucceſſively in weight.

The muriate of copper diminiſhed during 45 days before it began to encreaſe. Theſe oſcillations and retrograde movements take place but once, and when the ſalt has abſorbed a certain quantity of water, there is a progreſſive increaſe, although ſlowly, until its perfect ſaturation, which may depend on the attraction of water for water, an attraction which is not perceptible but in certain proportions.

Expediency of further experiments.

Theſe anomalies deſerve to be obſerved again, and compared with experiments made on other ſalts which do not exhibit them. They tend to make us acquainted with all the cauſes that produce efflureſcence and deliqueſcence, ſince they preſent each phenomenon ſucceſſively. The ſalts which we ſubmitted to their action, had certainly an attraction for water very little different from that of air in a medium ſtate of heat and humidity. The point of equilibrium muſt be decided by the ſtate of the atmoſphere, or the ſalts would remain unaltered.

Theſe will probably ſhew that meteoric variations in the air

I ſtill however think that a relation exiſts between the meteorological variations and the alterations of the ſalts; and if I

was

was not able to diſcover it, without doubt this was cauſed by the ſmall portions of ſalts which I expoſed to the action of the atmoſphere. Some chemiſt more fortunate will determine it, by operating on large maſſes, comparing experiments made in many different ſeaſons, and keeping a regiſter of the electrical ſtate of the atmoſphere, of the water of cryſtallization which the ſalts contain, of their diviſion, and of the ſurface which they preſent to the air.

do influence th changes in ſalts

In a labour which would require more than 3000 experiments, the new facts which I have obſerved are too few, and perhaps too little important to engage any one to undertake ſuch prolonged and minute experiments; but I have given a table of deliqueſcent ſalts arranged according to their attraction for water, and I dare hope, that the reſults of it will not be altogether uſeleſs.

Extenſive reſearch.

IV.

Account of the ſimple and eaſy Means by which the Harbour of Rye was reſtored, and made navigable for Ships of conſiderable Burthen. By the Rev. DANIEL PAPE *.

Memorial of Rye Harbour.

RYE Harbour, once ſo very ſafe and convenient for paſſing veſſels up or down the channel, to run to in diſtreſs or in precarious weather, had been for many years, and from various cauſes, in a gradual ſtate of decay, inſomuch that in the years (I believe) 1795 and 1796, it was thought neceſſary to ſend Captain ———, from the Trinity-Houſe, to make a ſurvey, and report to the Board its then ſtate, and the probability of its improvement or redemption. The ſurvey was made, I believe, with conſiderable care and attention; and the reſult was, that the harbour was pronounced loſt, or in ſuch an irreparable decayed ſtate, that it was an uſeleſs expenſe to the ſhips paſſing, which paid tonnage to it; and therefore this tonnage was taken from Rye, and given to Ramſgate Harbour, leaving however a reſerve in the hands of the commiſſioners of 6000*l.*

Decayed ſtate of Rye Harbour, in 1796.

Survey and report,

that the Harbour was irreparable.

* From his communication to the Society of Arts, who voted him the gold medal. See their Tranſactions, Vol. XXII.

The

Advertizement for plans of improvement.

The conſequence of this was an advertiſement, inviting any gentleman to come forward with plans for the improvement of the harbour, and the draining of the upper levels. On the day appointed for the preſentation of ſuch plans, a very ſenſible letter was laid before the Commiſſioners by the Rev. Mr. Jackſon, of Rye, though impracticable on many accounts,—and alſo a plan by myſelf, propoſing to make the preſent cut, and to form a dam of ſtraw or hay and faggots, as repreſented on the chart, for the ſmall ſum of 500*l.* On reverting to the *enormous ſums* that had been already, from time to time, expended by able engineers to no purpoſe, it was judged at the moment an impoſſible attempt; and, after politely voting me their thanks, the Commiſſioners ſeemed to decline carrying their plan into execution.—This, however, did not ſatisfy me; and therefore, confident of ſucceſs, I undertook to perform what I had propoſed, or loſe the money, without ſtipulating for any fee or reward ſhould I ſucceed. On entering upon this agreement, I ſet to work, and chooſing a Mr. Southerden, an active and perſevering man, as foreman, to aſſiſt me, I completed the work in three months, in the very depth of winter, at the expenſe of only 480*l.* though the works were twice filled up with ſea-beach by the tides. But, though this was done to the aſtoniſhment and admiration of many, yet there were evidently an envious few mortified and diſappointed. The cut and dam being thus finiſhed, it was then thought neceſſary, on my recommendation, to ſecure the cut from reverting to its late reduced ſtate, by a pier-head on the eaſt, and jutties on the weſt ſide of it; the execution of which was committed to the eminent ſkill of a Mr. Sutherland, who performed the truſt repoſed in him, to the univerſal ſatisfaction of his employers; and I believe the whole was completed for ſomething leſs than 3000*l.*, in a very maſterly and workman-like manner. Of this I think there cannot be a better proof adduced, than that it ſtill ſtands *firm*, without the leaſt apparent decay, and maintains its firſt poſition without the ſmalleſt variation: and no doubt a very trifling annual expenſe will keep it in its preſent improved ſtate.

The author's plan to make a direct cut, and dam up the old mouth,

The author undertook it at his own riſque,

and completed it.

Farther ſecurity by a pier head and jutties.

It proves to be perfectly durable,

and admit ſhips of five times the tonnage before admitted.

The advantages derived from it are particularly great; for ſhips of 250 tons burden, and even veſſels of 300 tons, run in with the greateſt ſafety at ſpring tides: whereas, before, thoſe of 50 tons could not come in, but with the utmoſt difficulty and danger.

That

That part of Romney Marſh too, which lies contiguous, and was threatened by every boiſterous tide with a total overflow, is now in ſafety, and the drainage of the levels is rendered complete. Other advantages.

I beg leave now to offer to your attention a ſhort deſcription of the Dam, the form and materials of which may be uſed with ſucceſs in ſimilar ſituations, whether in places adjacent to the ſea, or in gentlemen's fiſh-ponds, or rivers in the country, where weirs may be neceſſary for the preſervation of the banks. The dam was merely formed of hay, ſtraw, and faggots, pinned down to a *foundation* of *ſand* or *ſilt* by ſhort piles. I formed it as in the chart, of the ſhape of a double-roofed houſe, firſt putting down ſtraw, and then over it hazel faggots, from 12 to 14 feet in length, and afterwards pinning down the whole with piles. I next filled the ſpace between the two roofs with gravel or ſea-beach, and ſecured this alſo with faggots pinned down upon it, over which reſiſtance being precluded from its peculiar form, the influx and reflux of the tides glided ſo gently, that conſequently every probability, not to ſay poſſibility, was annihilated of its being ever undermined or blown up. Very eaſy and beneficial method of conſtructing the dam. A double roof or covering of hay, &c. was pinned down upon a foundation of ſand, &c. The interſtice was filled with gravel, and covered with faggots ſecured down.

It was alſo neceſſary that this dam ſhould be put down in one tide, and that the mouth of the cut ſhould be opened in the ſame time; for it was evident to me, that it was impoſſible ever to cut to ſea in any other way. For unleſs the dam had been ready to turn the water through the cut as ſoon as opened, and the cut, on the other hand, ready to receive the current the moment the dam began to act, the whole work muſt have been entirely and unavoidably deſtroyed by the influx and reflux of the enſuing tide. All this I clearly foreſaw: and by procuring a ſufficient number of men, nearly three hundred, the buſineſs was completely finiſhed, juſt as the tide touched the foot of the dam; and when it was full ſea, the ſtraw of courſe acted as a receiver and retainer to the *ſilt* brought in by the tide; which being repeated by each returning tide, the dam ſoon became entirely fixed, beyond a poſſibility of ever being deſtroyed; and it is now ſo entirely covered, that if the pier is kept in repair, the dam muſt ever remain unimpaired by time, and proof againſt the moſt violent floods of waters. Difficulty that the old mouth ſhould be cloſed and the new one opened in one ſingle tide. Succeſsful reſult.

For this work, the Commiſſioners voted me fifty guineas (half of which I gave to my aſſiſtant) and alledged that, on account The author's reward for his contrivance, and

attending the works: 50l.! account of the loſs of the tonnage, and the poverty of the fund, they were ſorry it was not more. This to me, under theſe circumſtances, was a ſufficient apology, and I was content. I now offer it to the conſideration of the Society of Arts, as a body in ſome degree intereſted in the proſperity of this kingdom. Should they deem what I have already received an adequate compenſation for ſuch a work, and ſuch an undertaking, at ſo inclement a ſeaſon, I am ſtill content. But if they ſhould think proper to grant me an additional remuneration, it will be received with peculiar ſatisfaction, and conſidered as a very great honour by,

Sir,

Your obedient and humble Servant,

To Charles Taylor, Eſq. DANIEL PAPE.

Cambridge, Trinity Hall,
April 2, 1803.

Reference to the Engraving of the Rev. Mr. PAPE'S *Improvement of Rye Harbour, Plate* XIII. *Fig.* 1.

AA. The double roof, filled with ſtraw.

BBB. Hazel faggots, 12 to 14 feet long.

C. The ſpace betwixt the roofs filled with gravel or ſea-beach.

D. The faggots which covered the gravel ſo laid.

E. Piles of wood driven through the faggots and ſtraw into earth, at the bottom of the river, the heads of which piles are united by croſs pieces of wood.

F. The ſolid bed of the river.

G. The river at low water.

H. The high-water mark.

I. The upper ſide of the dam, which oppoſes the current of the river.

K. The lower ſide of the dam, which reſiſts the coming-in of the tide.

Fig. 2. L. Shows the place where the dam was placed.

M. The old courſe of the river repreſented by dotted lines, and which is now filled up with gravel by the tide.

N. The new canal, cut by Mr. Pape's directions, and which is now the regular channel for ſhipping.

O. The pier-head, on the eaſt ſide of Mr. Pape's cut.

PP. The two jutties, on the weſt ſide of Mr. Pape's cut.

RR. The former canal, cut under the direction of Mr. Smeaton, and other able engineers; but which failed, and is ſince blocked up by a bank made acroſs it, over which the preſent high road between Rye and Winchelſea paſſes.

V.

New Experiments on the Reſpiration of Atmoſpheric Air, principally with regard to the Abſorption of Azote, and on the Reſpiration of the Gaſeous Oxide of Azote. By *Profeſſor* PFAFF, *of Kiel* *.

THE great diſcoveries in pneumatic chemiſtry, the ingenious and uſeful applications of theſe diſcoveries to explain the phenomena of organized beings, particularly the animal economy, and the valuable reſearches of eminent philoſophers have greatly contributed to throw light on the doctrine of the chemical effects of reſpiration. In conſequence of theſe reſearches, phiſiologiſts are in general agreed with regard to the moſt eſſential points of this doctrine; ſuch as the production of carbonic acid, the uſe of oxigen gas, and the animal heat which reſults from its abſorption. But the activity of philoſophical enquirers has not yet ſucceeded in removing all the obſcurities of this ſubject, and the diſagreement between the reſults of various experiments relating to them, ſufficiently ſhew, that new enquiries are requiſite to aſcertain the ſources of ſuch errors as ſtill continue, and to remove them altogether. The experiments of the celebrated Davy have done much in this reſpect, and the reſcarches on the nitrous oxide afford a new epocha in the chemical doctrine of reſpiration. The celebrated editors of the Bibliotheque Brittannique, have ſhewn their conviction of the great value of theſe reſearches, by the ample and inſtructive extract they have given, and the manner in which Berthollet in the 45th volume of the *Annales de Chemie* has given an account of the ſame, has ſufficiently fixed the attention of philoſophers on that excellent work.

Short hiſtory of chemical reſearches concerning reſpiration.

Davy on nitrous oxide.

* This memoir was addreſſed to the French National Inſtitute, and read at their ſitting of the 25th of Meſſidor laſt (July 13.)

The

The differences which exiſted in the reſults of former experiments, as to the quantity of carbonic acid produced in the act of reſpiration were leſs important, and might entirely depend on the conſtitution of the different individuals upon whom the experiments were made; and under this point of view, a reviſion of the experiments was leſs neceſſary. But the part which is performed by azote gas in the act of reſpiration has been too little attended to. It has been generally ſuppoſed to be altogether without activity. Goodwin alone thought he had obſerved a conſiderable abſorption of azote gas; but his experiments were not made with all the neceſſary accuracy, and were too directly oppoſite to the experiments of Lavoiſier, Seguin, Abernethy, Fothergill, Menzies, &c. to fix the attention. The experiments on the ſlow combuſtion of phoſphorus, which does not ſucceed in pure oxigen gas, but is ſo greatly forwarded by the preſence of the azote gas of atmoſpheric air, ſhew to a certain degree the advantages which this great quantity of azote gas is likely to produce in reſpiration; and the unfortunately too conciſe reſults of the laſt experiments of the immortal Lavoiſier on reſpiration, in which it was found, that a much greater maſs of oxigen gas is decompoſed in the ſame time by reſpiration in atmoſpheric air than in oxigen gas, ſtand in confirmation of the former fact. But hitherto we have poſſeſſed only probabilities, or reſults not ſufficiently connected with the ſubject. To Davy it is that we are indebted for an exact and inconteſtible knowledge of the active part which azote gas performs in the proceſs of reſpiration. But in proportion to the novelty and intereſting nature of theſe reſults do they require to be confirmed by the experiments of others; and it was in this point of view that I undertook laſt winter a ſeries of experiments upon reſpiration in atmoſpheric air, and alſo in the gaſeous oxide of azote; the principal reſults of which I now venture to communicate to the National Inſtitute.

Azote neceſſary for the ſlow combuſtion of phoſphorus.

Experiments on the Reſpiration of Atmoſpheric Air, and Oxigen Gas.

Experiments of reſpiration.

All the following experiments were made in the academical laboratory of the Univerſity of Kiel, which is provided with all the accurate apparatus of modern chemiſtry. They were made

made for the moſt part in the preſence of my pupils, particularly one named *Dierks*, who was moſt commonly the ſubject of experiment.

In order to determine with preciſion the changes which atmoſpheric air undergoes by reſpiration, and to decide reſpecting the abſorption of azote gas, we muſt begin with aſcertaining the diminution which a given volume of air undergoes by reſpiration. This firſt point was to be determined by accurate experiments.

Quantity of diminution undergone by air by the proceſs of reſpiration.

1. The quantity of 170 duodecimal cubic inches of Paris* were reſpired from one of the great reſervoirs of a gaſometer, conſtructed at Paris after the model of that of Charles, over water covered with oil, to prevent the abſorption of the carbonic acid gas produced by reſpiration. The reſpiration was performed once only during the time of ten or twelve ſeconds. The diminution was 4,72 cubic inches, or $\frac{1}{36}$ part of the firſt volume. This experiment being repeated twenty times in the ſame manner, afforded the ſame reſult.

2. 144 Cubic inches were reſpired once in the ſpace of ten or twelve ſeconds. The diminution was four cubic inches or $\frac{1}{36}$ part of the primitive volume.

3. The ſame volume was reſpired twice during 22 ſeconds, and the diminution amounted to eight cubic inches, or $\frac{1}{18}$ part of the primitive volume. The ſame volume having been reſpired three times during 30 ſeconds, the diminution amounted to 12 cubic inches, or $\frac{1}{12}$ part of the primitive volume.

4. 60 Cubic inches were reſpired three times during 25 ſeconds, the diminution was ſix cubic inches, or $\frac{1}{10}$ of the primitive volume.

5. 170 Cubic inches were reſpired four times during one minute, and the diminution amounted to 20 cubic inches. This experiment was ſeveral times repeated, and the diminution was almoſt conſtantly the ſame. Namely, 18, 19, 21, and 20 cubic inches, or $\frac{1}{27}$.

6. 168 Cubic inches reſpired during 50 ſeconds, by four great and four ſmall reſpirations, ſuffered a diminution of 14, or $\frac{1}{12}$ of the primitive volume.

7. 430 Cubic inches by 12 reſpirations in 90 ſeconds, ſuffered a diminution of 24, or $\frac{1}{18}$ part.

* As theſe quantities are merely relative, I have not reduced them. T.

Theſe

Theſe reſults agree very well with thoſe obtained by Davy on the diminution of air by reſpiration. He found the diminution by one ſingle reſpiration to be $\frac{1}{45}$ part, and by reſpiration continued for one minute $\frac{1}{18}$ part.

The magnitude of the diminution depends not only on the time during which a given volume of air is reſpired, but principally on the magnitude of this volume itſelf; it muſt be proportionally leſs the greater the quantity of air inſpired. A very eſſential error is ſeen in the reſults of Abernethy, who gives a greater volume to the expired than to the inſpired air; and the calculations of Goodwin are founded on a miſtaken baſis; for he ſuppoſes the two volumes equal.

Diminution of oxigen gas by reſpiration.

In order to determine comparatively the diminution of oxigen gas by reſpiration, 170 cubic inches of oxigen gas obtained from manganeſe were reſpired in the ſame manner, and under the ſame circumſtances as the 170 cubic inches of atmoſpheric air in the 5th paragraph. The diminution amounted to 30 cubic inches, and in other experiments, to 33, 29, 31. The mean term of which is $\frac{2}{11}$ parts of the primitive volume.

This diminution being eſtabliſhed with accuracy, may be applied to determine the abſorption of azote gas.

Experiments to determine how much azote is abſorbed in the proceſs of reſpiration.

8. 80 Cubic inches were reſpired one time ſlowly during ten or 12 ſeconds, and the air expired was received over mercury.

The relative quantity of the conſtituent parts of this reſpired air was in the centenary 4,16 carbonic acid, 16,55 oxigen gas obſerved by the ſlow combuſtion of phoſphorus, 79,19 of azote gas. An eudiometric experiment made at the ſame time, gave the following proportion of the parts in atmoſpheric air, one carbonic acid, 21 oxigen gas, and 78 azote. The total diminution of the air was from the preceding experiments $\frac{1}{36}$. We may therefore find the true quantity of azote gas by the following proportion, 36 : 35 :: 79,19 : 76,99. If we ſubtract this 76,99 from 78, the primitive quantity of azote in the atmoſpheric air before reſpiration, we find a loſs of 1,01 on the hundred parts of the whole maſs of air breathed. But as the quantity of air inſpired was really no more than 80 cubic inches, the abſolute diminution or diſappearance of azote gas by one reſpiration, muſt

muſt be diminiſhed in the ſame proportion of 100 to 80, and thus proves 0,803 cubic inches.

9. In another experiment 60 cubic inches were reſpired once in the time of 10 or 12 ſeconds, and the laſt portion of the expired air was received over mercury. The proportion of the conſtituent parts after reſpiration, were in the centenary 4,68 carbonic acid gas, 17,68 oxigen gas, and 77,74 azote gas. An eudiometric experiment made at the ſame time on the atmoſpheric air, gave 1 carbonic acid, 22 oxigen gas, and 77 azote gas. The true quantity of azote gas found as before, by diminiſhing the 77,74 $\frac{1}{30}$, is, 75,58. And this being ſubtracted from 77, the quantity of azote gas previous to the reſpiration leaves 1,42 for the azote which diſappeared, ſuppoſing the reſpired air to be divided into 100 parts. But if we take the real number in inches, which was 60, this quantity will be expreſſed by 0,852 cubic inches.

10. 30 Cubic inches were reſpired in the ſame manner three times during 16 ſeconds. The expired air contained in the centenary 5 carbonic acid gas, 14,5 oxigen gas, and 80,5 azote gas. The atmoſpheric air contained by experiment at the ſame time, 1 carbonic acid gas, 29,75 oxigen gas, and 80,025 azote gas. This by the ſame proceſs of computation gives a diminution of 4,235 in the 100, or in cubic inches 1,2705.

Theſe experiments which were ſeveral times repeated, and conſtantly with the ſame reſult, deciſively ſhew that azote gas is abſorbed in the act of reſpiration, and the active part it performs. Hence we may more eaſily underſtand, why azote gas compared with other mephitic gaſes is ſo little noxious to our lungs; ſo that according to the experiments of Lavoiſier and Seguin, animals live very well in a mixture of 15 parts azote gas, and one part oxigen gas; whereas the ſame animals were ſuddenly ſuffocated in a mixture of 40 parts oxigen gas, 45 azote gas, and 15 carbonic acid gas. Hence we may comprehend, at leaſt to a certain extent, the extraordinary effects of the gaſeous oxide of azote; we may form ſome notion of the transformation of the chyle, which is leſs anamalized or azotized in the lymphatic part of the blood, but becomes more ſo in the act of reſpiration. But the quantity of azote gas abſorbed by one ſingle reſpiration is not very conſiderable, which

Remarks on uſes, &c. of azote.

which agrees perfectly with the experiments of Davy, who found that no more than 5,1 cubic inches of azote gas were absorbed by 19 respirations of a volume of 161 cubic inches.

Experiments shewing the quantities of carbonic acid produced in respiration.

11. To determine the quantity of carbonic acid gas produced by the respiration of atmospheric air, 60 cubic inches were respired once during ten or twelve seconds, and received over mercury when expired. Lime water absorbed 4,68 parts in 100. This experiment being several times repeated gave the same result. The last portion of expired air being several times transferred through lime water was diminished 4,9 parts in 100.

12. 20 Cubic inches respired three successive times during 10 seconds afforded no more than five hundredths of carbonic acid gas.

13. 170 Cubic inches were respired four times during 50 seconds, the quantity of carbonic acid gas obtained was 5,8 hundredths.

14. 170 Cubic inches were respired from a bladder eight times in one minute. Lime water absorbed 8,2 hundredths.

This quantity of carbonic acid produced by respiration, afforded a term of comparison to ascertain the quantity of the decomposition of oxigen gas in respiration from the same quantities of atmospheric air, and of pure oxigen gas.

Oxigen gas produces more carbonic acid in respiration than atmospheric air does.

The preceding experiments (7) had shewn that the diminution of oxigen gas was more considerable than that of atmospheric air. From this fact it might be expected, that the production of carbonic acid gas would likewise be more considerable; and this was confirmed by direct experiments.

15. 170 Cubic inches of oxigen gas obtained from manganese, were respired four times during 50 seconds; the diminution was 30 cubic inches. The quantity of carbonic acid produced was 8,2 hundredths. Atmospheric air respired in the same manner, and under the same circumstances, gave only 5,8 carbonic acid.

16. 70 Cubic inches respired from a bladder during 50 seconds, also gave eight hundredths of carbonic acid.

Experiments on the Respiration of the Gaseous Oxide of Carbon.

Observations on the method of obtaining gaseous oxide of azote.

The gaseous oxide of azote was obtained by the process of Davy from crystallized nitrate of ammonia. This nitrate of ammonia affords very different products in different temperatures.

peratures. I have made a conſiderable ſeries of experiments on this ſubject, which I ſhall ſhortly ſubmit to the National Inſtitute. I ſhall only remark in this place, that the oxigenated muriatic acid is obtained at the commencement, if the nitrate of ammonia be not entirely free from muriatic acid; that at a temperature not exceeding 220 degrees of the centigrade thermometer, the gaſeous oxide of azote is obtained in great quantity, and very pure, without any mixture of thoſe white vapours which have the taſte of muſtard; but that a temperature ſtill higher, eſpecially at a red heat, the gaſeous oxide of azote is no longer diſengaged but nitrous gas is formed, and very peculiar white vapours which I am at preſent examining. To prevent any exploſion, I always mix the nitrate of ammonia with very pure ſand. To obtain the gaſeous oxide of azote in a very pure ſtate, the diſtillation muſt be made on a ſand bath, and the fire carefully managed. When every thing ſucceeds properly, the gas is ſo pure, that it may be reſpired immediately; it has an agreeable taſte, almoſt ſacchar vinous. If it be mixed with the white vapours produced by too ſtrong a heat, time muſt be allowed for them to be depoſited. The effects which Davy has obſerved, and Pictet has deſcribed with ſo much intereſt in his ſecond letter in the 17th Volume of the *Bibliotheque Britannique*, were perfectly confirmed in my experiments. Several perſons who reſpired this gas were exalted abſolutely in the ſame manner. One of thoſe who reſpired it was very ſpeedily intoxicated, and put into a very extraordinary and moſt agreeable extacy. Others reſiſted ſomewhat longer; one only ſeemed to be ſcarcely at all affected. The exaltation always paſſed over without leaving any perceptible relaxation. I ſtill continue theſe experiments. Perhaps this gas may become a powerful remedy for melancholy affections. I ſhall not fail to communicate the reſults of my experiments to the National Inſtitute.

Davy's experiments ſucceeded perfectly with our author.

VI. *Experiments*

VI.

Experiments on Gum Arabic and Gum Adracanth. *By M.* VAUQUELIN *.

Red gum adracanth left after combuſtion $3\frac{1}{2}$ hundredths chiefly carbonate of lime, with a little iron and phoſphate of lime.

TEN grams of red gum adracanth produced on combuſtion three decigrams and a half of white aſhes. Theſe aſhes diſſolved in muriatic acid with effervescence, and gave forth an odour of ſulphurated hidrogen. Their ſolution depoſited a precipitate by ammonia, which was phoſphate of lime and oxide of iron. The oxalate of ammonia precipitated from it much lime. Thus red gum adracanth contains in 100 parts about $3\frac{1}{2}$ of aſhes, which was compoſed for the moſt part of carbonate of lime, a ſmall quantity of iron, of phoſphate of lime, and perhaps of a very minute portion of alkali.

White gum adracanth left a reſidue of 3, which contained the ſame principles and alkali.

2. Ten grains of white gum adracanth ſubmitted to the ſame proofs, gave three decigrams of aſhes, which were compoſed of the ſame principles as the red kind, with the addition of a little potaſh.

Gum arabic left 3 containing no alkali.

3. Ten grains of gum arabic burnt as the others, left three decigrams of aſhes, which were compoſed of the ſame elements as the preceding, except that they gave no ſign of the preſence of alkali or of ſulphur.

Opacity and difficult ſolubility of gum adracanth.

I formerly thought that the opacity of gum adracanth, and the difficulty of its ſolution in water, might be occaſioned by a greater proportion of earthy matter; but after theſe experiments it appears, that they are due to another cauſe.

The lime in gums is combined with acid, forming a ſoluble ſalt.

The lime which I found in the gum, which I am about to mention, was doubtleſs neither in the ſtate of carbonate, and ſtill leſs in that of quicklime; for the ſolutions of the gum were not in the leaſt alkaline, but on the contrary, ſlightly acid; at leaſt a bit of the gum rubbed on ſome paper well moiſtened (with blue vegetable juice) made it ſenſibly red. It is alſo certain, that oxalate of ammonia and carbonate of potaſh occaſion precipitates in the ſolution of gum arabic, and that acetite of lead does not form any. It follows from this, that the lime is moſt probably united to ſome acid in the gums, which doubtleſs is a vegetable acid; for in being decompoſed they leave their baſes combined with carbonic

* Annales de Chimie, Tom. 54.

acid;

acid; but can be neither the oxalic, the tartarous, or the citric, becauſe their combinations are inſoluble in water, and that beſides they exiſt but in a ſmall number of vegetables; ſtill leſs can it be the benzoic, the gallic, the *moroxalic*, or the *honeſtic*, which are very rarely found in naturally, and of which the three laſt alſo form inſoluble compound s.

There only remains to decide between the acetous and the malic acids, which are the moſt abundant in the vegetable kingdom. The firſt forms, as is well known, ſoluble combinations with all the ſubſtances with which they are capable of union; ſome of them are even deliqueſcent. It is beſides the moſt frequent reſult of the operations of nature in the vegetable and animal ſyſtems, ſince it is formed by vegetation, by fermentation, the action of the more powerful acids, and by the influence of heat.

The acid muſt be either the acetous or the malic.

The combinations of the ſecond are for the moſt part inſoluble in water; that which it forms with lime particularly, is not ſenſibly ſoluble, but when there is an exceſs of acid; and its exiſtence in nature is by no means ſo frequent as that of the acetous acid; and as the lime which is found in the tranſparent gums has been inconteſtibly diſſolved in the juices of the vegetables which produce theſe ſubſtances, it is much more probable, that this earth is in them combined with acetous acid than with any other.

But the malic forms inſoluble compounds with lime,

conſequently the acid is more likely acetous.

It is alſo probable, that the ſmall quantity of potaſh which I found in the aſhes of the burnt gums, is united to the ſame acid, which explains why theſe ſubſtances are ſo ſenſible to humidity, and ſoften ſo much as to prevent their pulverization.

The potaſh in gums alſo forms an acetite.

I am, however, much inclined to think, that in certain opaque adracanth gums, which are of difficult ſolution, and yield much lime on incineration, this earth is combined with malic acid. I have had occaſion lately to examine a gum gathered by M. Paliſſot Bauvois, from the cochineal nopal, which was opaque, ſwelled with water, did not diſſolve uniformly, and which yielded eight per cent. of lime. And as the ſap of every cactus which I have analyzed, yielded more or leſs acidulous malate of lime, there is great reaſon to believe, that the ſpecies of it which nouriſhes the cochineal contains it likewiſe: and that it is the preſence of this ſalt proceeding from the vegetable, and diſſolved in the ſap along

Some gums contain lime in greater proportions.

General refults.

with the gum, which cauſes its opacity, and obſtructs its ſolution in water. It reſults at leaſt from theſe experiments, that the gums contain, firſt a calcareous ſalt, moſt commonly the acetate of lime; ſecondly, ſometimes a malate of lime with an exceſs of acid; thirdly, phoſphate of lime; fourthly and laſtly, ſome iron which is probably alſo united to phoſphoric acid.

VII.

Method of obtaining Cobalt pure. By M. TROMSDORF *.

Zaffre detonated with charcoal and nitre.

FOUR parts of zaffre well pulverized are to be mixed carefully with one part of nitre, and half a part of charcoal in powder: this mixture is to be projected in ſmall quantities into a red hot crucible, and this operation repeated three times, adding each time to the reſidue new portions of the nitre and the charcoal.

Fuſion with black flux.

The maſs reſulting from theſe detonations ought to be mixed with one part of black flux, and expoſed during an hour in a crucible to a red heat.

The metallic button again detonated.

The whole is then to be left to cool; the metallic cobalt to be ſeparated, pulverized, mixed with three times its weight of nitre, and the mixture detonated with the precautions above mentioned.

Lixiviation ſeparates the acid and arſenic.

The iron contained in the cobalt will thus become ſtrongly oxidated, and the arſenic acidified combines with the potaſh: The maſs pulverized is to be lixiviated many times, and repeatedly filtered; and thus the arſeniate of potaſh formed will be ſeparated from the inſoluble reſidue that contains the cobalt.

Nitric acid diſſolves the cobalt alone.

This reſidue is then to be treated with nitric acid, which diſſolves the cobalt without attacking the iron which is found oxidated to its *maximum* of oxidation,

Evaporate and rediſſolve.

The ſolution is then to be evaporated to dryneſs, the reſidue rediſſolved in nitrous acid, and the liquor filtered, to ſeparate the laſt portions of the oxide of iron, which might have eſcaped in the firſt operation.

* From the Annals de Chimie, Tom. 65.

All

All that remains to be done after this is, to decompoſe the nitrate of cobalt by potaſh, to waſh the precipitate, and to effect its reduction by means of heat.

Precipitate the cobalt by potaſh and reduce.

VIII.

A new Method of extracting Sugar from Beet-Root.
By M. ACHARD *.

THE roots of the beet, after being properly cleanſed, are ſliced and preſſed. The juice obtained is thick, and of a deep colour: it contains, beſides ſugar, albumen, fecula, and other matters from which it muſt be cleared, in order to obtain the pure ſugar. In this proceſs of ſeparation it is, that the art of procuring ſugar from the beet-root conſiſts.

Beet roots are ſliced, preſſed, and the juice ſeparated.

In a boiler of tin, or of tinned copper, mix 100 lbs. of the juice of beet root with $3\frac{1}{4}$ ounces of ſulphuric acid diluted with one pound of water; then pour it off, and let it ſtand for 12, 18, or 24 hours; 12 hours are ſufficient, but 24 will not be detrimental to the proceſs, as the acid prevents any change in the juice. In order to ſeparate the ſulphuric acid, put into the liquor $7\frac{1}{2}$ ounces of wood aſhes, to which add ſoon afterwards, 2 ounces or $6\frac{1}{2}$ drachms of lime ſlaked in water. The ſulphuric acid coagulates the albumen, the wood aſhes, conſiſting chiefly of lime, and the lime itſelf ſeparate in their turn the acid, in form of an almoſt inſoluble ſalt. It will here be recollected, that in the Weſt Indies, in the fabrication of coarſe ſugar, and in the refining houſes of Europe, lime is uſed to aſſiſt the ſeparation, and the cryſtallization of this article.

One 26th part of ſulphuric acid is added to the juice, and after ſtanding 1-12th of wood aſhes, and one part of lime. The albumen, &c. are thus ſeparated.

After this firſt operation the beet root muſt be clarified; for which purpoſe it muſt be poured into a boiler ſo placed as that the fire may act equally upon all the whole ſurface of the bottom, in which it is to be heated to a ſtate bordering upon ebullition, but muſt not be ſuffered actually to boil. After drawing out the fire, the ſyrup is to be ſkimmed till the ſkum ariſes in blackiſh flakes. The liquor is now to be filtered through flannel, which muſt be done with caution, leſt the dregs paſs through with the ſyrup. The ſkum and the dregs are good for fattening ſwine.

Farther purification by boiling, ſcumming, and ſtraining.

* Van Mons's Journal, Vol. VI.

Brisk evaporation.

The syrup, thus clarified and filtered, is placed in a shallow cauldron, to the depth of not more than six inches, and evaporated over a brisk fire, whereby it is prevented from becoming a liquid saccharine mucus, which resists all attempts to crystallize it.

Cooling and farther purification by subsidence.

When reduced to about one half of its quantity, the syrup is to be poured into tin vessels about six feet in height, and half a foot in diameter, with cocks about six inches from the bottom. It must here be left for two or three days, during which time it precipitates whatever remaining impurities it may contain, particularly gypsum. At the end of this period, the liquid may be drawn clear off, and replaced in the shallow boiler, but only to the height of three inches, to evaporate; the fire to be gradually augmented, as the syrup thickens, until it be in a state of ebullition. The fire is then to be damped to prevent the sugar from burning, which would render it unfit for crystallization.

Evaporation till the syrup draws a thread.

When the syrup becomes fibrous, the fire is to be extinguished.

Crystallization or graining in a warm apartment.

In about half an hour afterwards, the syrup is to be poured into cones, of which the mouths are stopped with linen cloths, and containing a little coarse sugar-candy, grossly pounded. These cones are set in a room whose temperature is from 10° to 20° of Reaumur.

When the several operations have been dexterously managed, the sugar will crystallize in 24 hours: but if the evaporation, or baking, has been too hasty, the whole becomes a granulous mass, with the interstices filled with melasses.

The melasses drawn off

When the sugar is well crystallized, the mouth of the cone is to be opened, and an earthern vessel placed under to catch the melasses: this operation, according as the syrup has been more or less baked takes three or four weeks.

leave coarse sugar.

The substance remaining in the cones, of a yellow colour, more or less tinged with white as the baking has been well or ill conducted, in granulated crystals of various sizes, is the coarse sugar of beet-root.

Improvement.

Mr. Achard, in order to save time, and to avoid the use of vessels for settling the liquor, afterwards deviated from his original plan, by adding to the syrup, when half evaporated as above described, for every twelve quintals of roots used, five

five quarts of ſkimmed milk, and ſhortly afterwards one quart of vinegar. He then proceeded with the ſecond evaporation in the boiler.

This ſugar by refining may be made to anſwer all the uſes of that of the Weſt Indies, and may be rendered equally white by the uſual proceſs. Subſequent refining as uſual.

IX.

On Nickeline (Niccolanum), *a Metal in many Reſpects reſembling Nickel, lately diſcovered by Dr.* J. B. RICHTER.*

I HAVE long ſince conjectured in analyſing the cobalt ores of Saxony, that they contained, beſides cobalt, arſenic, copper, nickel, and iron, another metal which reſembled nickel in many f its properties, but the means which I have hitherto employed to ſeparate it did not before afford me any ſatisfactory reſults. Suſpicion of a new metal in cobalt.

I was chiefly ſurpriſed that nickel, after being purified by the liquid proceſs from cobalt, iron, and arſenic, and after that reduced without the addition of a combuſtible body, never formed a maſs, but was always found diſperſed in ſmall particles in a hard heavy maſs, which had the appearance of the remains from vitrified copper, Remarkable fact, that nickel cleared of iron and arſenic and fuſed, is almoſt diſperſed in ſmall globules through a hard, heavy, blackiſh-grey maſs.

This hard matter had no metallic luſtre, neither was it attracted by the magnet: Its colour was of a blackiſh grey on the ſurface, with a ſmall degree of brightneſs; and in powder it was brown, greyiſh, and greeniſh.

Some weeks ago I endeavoured to reduce *per ſe* almoſt half a pound of oxide of nickel, which I had purified as well as poſſible by the liquid proceſs, for the greateſt part of a year, at a conſiderable expence: as this oxide was not of a lively green, I thought this was cauſed by the "extractive matter" which might be in the potaſh employed for the precipitation of the ſulphate of nickel from the ammoniacal preparation: it is true that this triple combination had not that beautiful graſs-green colour which it commonly had; but I thought this Experiment with a large quantity of the oxide of nickel.

* *Annales de Chimie*, LXIV.

might

might be cauſed by the ſubſtitution of the potaſh to the ammonia mixed with the copper, which could not be ſeparated but by the reduction *per ſe*.

Only a ſmall quantity of nickel in a maſs was obtained.

From theſe ideas I hoped to have at leaſt four ounces of perfectly pure nickel, but was diſagreeably ſurpriſed by finding in the crucibles, which were deformed in the uſual manner, and perforated by the vitrified copper, a rough maſs with the appearance I have before mentioned, and which contained only a morſel of about two and a half drams, and conſequently only five drams of pure nickel in the two crucibles. I reduced to powder in an iron mortar the remaining maſs (which could not properly be called ſcoriæ), and ſeparated from it by the ſieve and the magnet, the particles of nickel which it might contain, which produced near two and a half drams more; and that nothing might be loſt, I treated the powder with nitric acid, which attacked it vigorouſly at the firſt, and gave a ſolution of nickel, but after that did not act on it in the leaſt, ſo that the powder was but little diminiſhed in weight: in expoſing this matter to reduction *per ſe*, it produced no regulus, but merely agglutinated its parts.

The denſe accompanying matter was then pulverized and the nickel ſeparated by the magnet and by nitric acid. Strong heat applied to the maſs gave no more regulus.

The maſs being again powdered was urged with charcoal, and afforded more than half its weight of metal in one maſs.

Having again pulverized the maſs, which weighed almoſt $4\frac{1}{2}$ ounces, I mixed with it one ounce of charcoal in powder, and expoſed to the fire of a porcelain furnace during eighteen hours, in a crucible cloſed with a luted cover, in a part of the furnace which ſeemed to me to have moſt heat. After having broken the crucible, which was in a ſound ſtate, I found, under a ſcoria of a deep blackiſh-brown colour, a well fuſed button of metal which weighed two ounces and three quarters: it was not at all connected with the adjoining parts of the ſcoria, and had at its inferior part a particular ſhape, which was cauſed by cavities which were not produced by the crucible.

It was ſteel colour, rather hard, ſcarcely malleable, magtical, &c.

This metal had the grey colour of ſteel, inclining a little to red: it preſented in its fracture a grain not very fine: it was rather hard: could be extended a little under the hammer in a cold ſtate: heated to redneſs it endured little the ſtrokes of the hammer: it was attracted by the magnet, but not ſo ſtrongly as either iron or nickel: it had many properties common to nickel, but it was diſtinguiſhed from it entirely by others. As many of theſe properties were ſuch, that thoſe

not

not well acquainted with nickel in its perfectly pure ſtate might take it for that metal, I have called it Nickeline *(Niccolanum.)*

Name *Nickeline.*

The nickeline was free from all the metals which are found in the cobalt ores, except a little copper.

The ſpecific gravity of caſt nickeline, which enters more readily into fuſion than nickel, is 8,55 ; and of forged nickeline 8,60. On putting it into nitric acid and heating it, it is attacked more quickly than nickel: I remember having obſerved an equally violent action of nitric acid on nickel reduced by charcoal, which I then conſidered as pure, and which I diſſolved in order to precipitate from it by potaſh an oxide, which I might reduce *per ſe*.

Specific gravity 8,6. Nitric acid diſſolves it.

The ſolution of the nickeline went on well; being come to the point of ſaturation, it had a blackiſh-green colour, and aſſumed a gelatinous conſiſtence.

I employed my firſt care to ſeparate from it a part of the iron which I thought it contained, and left it to dry a little over a ſpirit lamp: the maſs became continually of a deeper green, and in approaching to dryneſs it gave out much red vapours, and the reſidue became of a blackiſh grey; I added diſtilled water to it, which diſſolved but little of it, and that which was diſſolved was an inſignificant quantity of nickel.

Separation by drying of the acid. Reſidue a black powder.

I poured muriatic acid on the blackiſh powder well waſhed, which gave a green ſolution, in diſengaging a ſtrong odour of oxigenated muriatic acid.

Soluble in muriatic acid. Green ſolution; which when dried gave a reddiſh maſs that turns green by damp.

The muriatic ſolution was, as well as the nitric ſolution, of a deep blackiſh graſs-green colour: being evaporated to dryneſs, it produced a reddiſh maſs, which became green in a moiſt air, and which communicated the green colour to water in which it was diſſolved.

This dark-coloured oxide of nickeline was inſoluble in nitric acid, and in ſulphuric acid; but if ſugar or alcohol was added, the ſolution took place with facility at the boiling point.

Dark oxide of nickeline not ſoluble in nitric or ſulphuric acids without combuſtible matter.

The ſulphate of nickeline, being combined with water, is alſo of a blackiſh green; but it aſſumes a pale red colour on being deprived of the water.

Sulphate of nickeline.

If carbonate of potaſh be added to the preceding ſolutions of nickeline, it occaſions a precipitate of blue carbonate of nickeline, inclining a little to grey and green, and of a pale tint: This combination is very light and ſoft, and diſſolves in the

Precipitate by carbonate of potaſh;

the acids with a ſtrong effervescence. I remember to have had, ſome years ago, this precipitate of a bad colour, and not then to have examined it, conſidering it as a mixture of iron, nickel, and arſenic, (which laſt continually made itſelf noticed by its odour of garlic): But at laſt I ſuſpected its nature.

by cauſtic potaſh;

If the ſolution of nickeline is decompoſed by cauſtic potaſh, it gives a precipitate which reſembles in its colour carbonate of chrome; that is to ſay, it is of a deep greeniſh-blue, which does not change when it is waſhed: being dried with a gentle heat, it aſſumes a pale colour, which becomes deeper when it is moiſtened with water.

by ammonia.

If any of the foregoing ſolutions of nickeline is mixed with ammonia to exceſs, the liquor aſſumes a pomegranate red colour, and remains tranſparent; which proves that it does not contain any iron, beauſe that this latter is not ſoluble in ammonia. By candle-light this ſolution is with difficulty diſtinguiſhed from that of perfectly pure nickel; but by day-light, this latter is of an amethyſt red colour, as I have elſewhere remarked.

Points of compariſon between nickeline and nickel or cobalt.

I ſhall now compare the principal properties in which nickeline reſembles altogether, or in part, nickel or cobalt, and thoſe in which it is diſtinct from them.

It reſembles cobalt—

Reſemblances of nickeline and cobalt.

1. By its property of ſuper-ſaturating itſelf with oxigen at the expence of the nitric acid, and thus forming a body which reſembles the black oxide of mangancſe with regard to its ſolubility in the acids: 2. By its property of not being reducible but by the intervention of a combuſtible body.

It differs from cobalt—

Differences between nickeline and cobalt.

1. By the blackiſh-green colour of its ſolutions, even when they are entirely neutralized. It is known that the neutral ſolutions of cobalt in the ſulphuric, nitric, and muriatic acids, are of a crimſon-red colour; and that the muriate of cobalt alone becomes of a greeniſh-blue on being deprived of its water: from whence it happens that an exceſs of acid produces this colour, becauſe it combines with the water: With the muriate of nickeline preciſely the reverſe takes place; when mixed with water it is green (although of a leſs beautiful colour than the cóbalt without water), and when deprived of its water it becomes reddiſh.—2. By the colour of

its

its carbonate: that of cobalt is of a beautiful poppy-blue, but the carbonate of nickeline is a bluiſh-green inclining to a pale grey.—3. By the colour of its oxide precipitated without carbonic acid: that of cobalt is of a deep blue, and changes on waſhing to a blackiſh-brown; but this oxide of nickeline is of a greeniſh-blue, and its colour does not change.

Nickeline reſembles nickel—

Reſemblances between nickeline and nickel.

1. By its ſtrong magnetic quality; although this is not ſo great as that of nickel.—2. By its malleability, which however is leſs than that of nickel.—3. By the deep green of its ſolutions; although this colour is not ſo beautiful as that of the ſolutions of nickel.—4. By the loſs of this green colour when its neutral combinations are deprived of water.—5. By the colour of the acid ſolution with an exceſs of ammonia, which cannot be well perceived by candle-light.

Nickeline differs very diſtinctly from nickel—

Differences between nickeline and nickel.

1. Becauſe it cannot be reduced without a combuſtible body added to it.—2. Becauſe nitric acid attacks and oxidates it more eaſily. Nickel is not near ſo readily acted on by the nitric acid if it is not mixed with the nickeline, which almoſt always happens with the magnetic nickel which is conſidered to be in a ſtate of purity, and which has not been reduced *per ſe* before my diſcovery.—3. It alſo differs from nickel by the property firſt mentioned of thoſe in which it reſembles cobalt.—4. By the colour of its combinations with the acids, when deprived of water: This colour in nickel is almoſt a buff *(chamois)*, and in nickeline a reddiſh, except in the nitrate of nickeline, which cannot be deprived of water without decompoſing it.—5. By the colour of the precipitates, mentioned in the ſecond and third articles concerning the properties wherein this new metal differs from cobalt, which are in thoſe of nickel of a green colour entirely different from thoſe of nickeline, which latter are of a much more agreeable green, eſpecially thoſe of the carbonate.

X.

Letter from G. CUMBERLAND, *Eſq. on a Project for extended Roads on the Principle of the inclined Plane.*

To Mr. NICHOLSON.

SIR, *Oct. 26, 1805. Weſton-ſupra-Mare.*

Account of rail-roads,

ABOUT ten years ago having frequent occaſion to remark, and ſuffer from, the miſerable ſtate of the roads from Staines to London during the winter ſeaſon, I ventured to propoſe (not having at that time either ſeen or heard of rail-roads) a plan which I called a *truck-road* for the whole of that ſtage, becauſe it was intended to convey all ſorts of goods and even carriages on trucks, going to town on one ſide of the old road and returning by the other.

communicated to Dr. Anderſon.

This plan I ſent ſome time after to Dr. Anderſon, with a drawing, for his *Recreations*, but by ſome accident it was miſlaid and loſt; and the reward of my trouble was the ſly ſneers of my grave Windſor neighbours, to whom it was known, accompanied with a ſort of pity for heads capable of propoſing ſuch eccentric inventions.

Time however revenged my cauſe, by ſhowing them the practicability of ſuch ſchemes in the progreſs of the Surry undertaking.

Another plan by an inclined road.

At the ſame time *another* plan of expeditious conveyance occurred to my mind, but which I was deterred from then producing owing to the cold reception my firſt contrivance met with—And as no one, as far as I can learn, has hitherto brought forward any improvement of the kind (although ſo very obvious that it might eaſily be ſuggeſted to the mind of a child who had heard of roads on inclined planes,) I take the liberty to recommend it to your patriotic publication, convinced that whatever may, at one time or other, be of ſervice to mankind, will be always ſure of a favourable reception at your hands.

Particular detail. Diſpatches may be rolled in a ſpherical caſe down a long inclined channel.

The plan I propoſe then is this:—That all diſpatches and poſt-letters may, wherever it is compatible with the inclination of the ground, be conveyed ten or fifteen miles to and from London by means of *iron or wooden ſhells of a globular form, rolling*

rolling in a cylinder of brick or ſtone. When cloſed and locked, a due momentum being given at a proper elevation, it is eaſy to ſee that their ſpeed and ſecurity muſt far ſurpaſs any other mode of conveyance that we at preſent know of; and all that would be neceſſary in addition to the machine would be to have proper beds of ſand or wool bags to blunt their projectile force at the end of their career.

I ſhall not at preſent enter into the diſcuſſion of the conſtruction of the tube-road, or go to a calculation of their expence; but if you think the bare hint worth publiſhing it will give me pleaſure, ſhould the idea be approved, to go into all the minutiæ of their utility in other reſpects, and the means of their ultimate accompliſhment;

Being always, Sir,

Your obliged humble ſervant,

G. C.

XI.

On the State of Provincial Societies for Scientific and Literary Improvement. By a CORRESPONDENT.

To Mr. NICHOLSON.

SIR,

BEING in the cuſtom of viſiting Aberdeen, in one of my laſt tours, I inquired if there were any Antiquarian or Literary Society, or Subſcription Library there, and was much ſurprized to find neither one nor the other; there is, I was told, an Athæneum, in which a good number of newſpapers, and ſome of the moſt reſpectable periodical publications, are taken in, and in a room above that, a circulating library; this laſt I knew to be the property of two very reſpectable bookſellers in Aberdeen, and I believe the former is alſo, but the two united by no means effect the utility of either a literary ſociety, or a *ſubſcription* library, *in which the books, &c. are the property of the members, and whoſe concerns, ſuch as chooſing and ordering books, and the like, are conducted by a committee, choſen out of the ſubſcribers.* Few of thoſe who know that there is no ſuch inſtitution there, when they conſider the reſpectability of the place, either in a commercial or literary view,

Great advantage that would reſult to the town of Aberdeen from the eſtabliſhment of a public library, &c.

view, but muſt feel greatly aſtoniſhed; and more particularly will the want appear, when it is alſo known, that in Montroſe, Arbroath, Dundee, and Perth, places much ſmaller than Aberdeen, and not poſſeſſing any college eſtabliſhment, there are *ſubſcription* libraries, *on the above plan*; nay, that Perth hath alſo an Antiquarian Society! Subjoined is a liſt of ſome other places in North Britain, enjoying the advantages of ſuch eſtabliſhments as I would recommend to Aberdeen; ſome of whom, it is obvious, have not near the proſpect of ſucceſs that that place could command.

Glaſgow, Paiſley, Greenock,
Kilmarnock, Linlithgow, Haddington.
On the borders of Northumberland, Dunſe and Kelſo,

The annual ſubſcription to none *of theſe is more*, in ſome caſes *not ſo much*, as to the Athæneum of Aberdeen and others, and they all poſſeſs very excellent and increaſing ſelections of books.

I am, Sir,

Your's reſpectfully,

A TRAVELLER.

York Hotel, Bridge Street,
Black Friars.

and to other reſpectable and opulent places.

P. S. I am ſorry to be informed that neither Inverneſs, Banff, or Peterhead, poſſeſs ſuch inſtitutions, particularly the firſt, which preſents ſuch an abundant number of objects to the antiquarian, and is ſurrounded by, and contains, ſo many gentlemen of diſtinguiſhed liberality, and ingenuity; at this place the northern meeting was eſtabliſhed for the avowed purpoſe o promoting intercourſe amongſt diſtant families, but how much more might be effected of general amelioration and comfort, by the eſtabliſhment of a Literary and Antiquarian Society, in which ſubjects connected with general improvement might be diſcuſſed, and books in chemiſtry, agriculture, and other more immediately uſeful parts of knowledge, collected.

Notice

XII.

Notice of certain Inſtances of waſteful Negligence in ſome Fiſheries in the North. By an ENQUIRER.

To Mr. NICHOLSON.

SIR, *London, Oct.* 10, 1805.

Inſtances of waſteful negligence in ſome fiſheries.

IT is mentioned in the Statiſtical Reports of Banff and Peterhead, that the fiſhermen there never think of carrying their fiſh along the coaſt ſouthward, which they might do, to Leith, in 24 hours, or with a good briſk wind to Berwick-upon-Tweed, or even Newcaſtle-upon-Tyne; but when their reſpective towns are ſupplied *they throw the remainder upon the dunghills for manure!* this was poſitively affirmed to me as a truth, by a gentleman of great reſpectability of Aberdeen.—At Arbroath another cuſtom equally as extravagant in its kind prevails, and of which I have been a witneſs: the crab fiſhery there is ſo productive, that after boiling them, *the bodies of the crabs are thrown away,* and the *large claws only* brought to table! Ought not ſuch amazing waſte to be remedied? *

Your's reſpectfully,

An ENQUIRER.

XIII.

On Bile. By M. THENARD †.

Bile conſidered as a ſoap with albumen.

BILE has been commonly conſidered as a ſaponaceous liquor charged with albumen: but it has been found, upon cloſer inveſtigation, to preſent phenomena which cannot be accounted

* *Qu.* What may the value of manure procured from fiſh at theſe places, compared with the price of the article at the neighbouring markets, ſubject to the deduction of carriage (coaſtwiſe), and the effect of a rival ſupply from nearer parts of the coaſt?—The facts which would ſolve this queſtion, would ſhew whether the fiſhermen neglect their intereſts in theſe proceedings.—W. N.

† Bulletin des Sciences, No. 95.

for

for merely by the preſence of theſe principles: this is more particularly to be obſerved on ſubmitting it to the action of fire and of acids.

Deſtructive diſtillation leaves one-eighth, in which is only one-fifth of ſoda; and this cannot ſaponify the oil.

Bile, if diſtilled to dryneſs, leaves a reſiduum equal to $\frac{1}{8}$th of its original weight; from 100 parts of which calcined is obtained a carbonaceous matter, compriſing ſeveral kinds of ſalt; as marine ſalt, phoſphate of ſoda, ſulphate of ſoda, phoſphate of lime, oxide of iron, and four parts of ſoda. Bile therefore contains no more than $\frac{2}{100}$ parts of its weight of ſoda.

So ſmall a portion of alkali would not be ſufficient of itſelf to diſſolve that quantity of oil which is known to exiſt in bile: a fair preſumption may therefore be entertained that this liquor contains ſome other property to ſupply the abſence of alkali. This conjecture increaſes to ſtrong probability, if not to abſolute certainty, in attending to the action of acids on bile.

Acid ſeparates oil and albumen from bile; the clear fluid is bitter, and affords by evap. a reſiduum.

If a few drops of acid be mixed with bile, a liquor of a reddiſh tint is obtained, which ſtains paper of a bright yellow. In this experiment little or no precipitation is perceived; but on the addition of more acid, it takes place abundantly: the matter depoſited conſiſts of albumen joined with a very ſmall portion of oil, not at all correſpondent to the quantity of theſe ſubſtances to be found united in pure bile. The liquor remaining after filtration is of an extremely bitter taſte, and leaves on evaporation a reſiduum equal to what is obtained from a like quantity of bile in its original ſtate.

The oil with an alkali and albumen is not bile.

On diſſolving the oil, which had been previouſly obtained from bile, in alkali, and adding to the ley produced, a portion of albumen, a combination took place which was decompoſed by the moſt feeble acids, and from which vinegar precipitated all the oil. This combination, therefore, was not bile; conſequently bile conſiſts not merely of albumen, oil, and ſoda; and this is the reaſon why ſoluble ſalts, barites, ſtrontian, lime, and ſeveral metallic diſſolvents, make no impreſſion upon bile.

Bile contains a peculiar matter.

No longer doubting that there exiſted in bile a matter peculiar to itſelf, I endeavoured to ſeparate it; and after a few trials, I ſucceeded, by means of a combination of acetous acid with lead.

Acetite of lead precip. the oil and alb. The liquor by evap.

On pouring into bile acetite with a ſlight exceſs of oxide of lead (that is, acetite of common lead boiled with about the 6th part of its own weight of litharge deprived of its carbonic acid)

acid) the whole of the albumen and oil were precipitated; the liquor being filtered, the oxide of lead and acetite were separated from it by means of ſulphurated hidrogen; and by evaporation, after having again filtered the liquor, a ſubſtance was obtained whoſe flavour was at once ſaccharine and acrid, ſomewhat ſimilar to the juice of certain kinds of liquorice. But as this ſubſtance was ſtill ſuppoſed to be charged with the ſalts of the bile, changed into acetite, by the acetite of lead, it was precipitated with acetite ſuper-ſaturated with oxide of lead, containing one part of the quantity of acid found in common lead; the precipitate was diſſolved in vinegar, to free it from the ſulphurated hidrogen, filtered, and again evaporated; by which means the matter was obtained in its greateſt purity.

gave a ſubſtance which when pure

Its principal qualities are:

had the peculiar qualities here enumerated.

1. Being ſoluble in water, and in alcohol, ſlightly deliqueſcent.

2. It is not precipitated by acetite of common lead; but is entirely ſo by the ſaturated acetite of lead, which precipitate is ſoluble in acetite of ſoda.

3. It will not ferment with yeaſt; will give no ammonia by diſtillation; and is not affected by the preſence of nutgall.

4. It diſſolves the oily matter of bile: but to facilitate this operation, it is neceſſary to diſſolve the two matters together in alcohol, evaporate, and waſh the reſiduum in water. One part of the ſaccharine and acrid ſubſtance diſſolves only three-fourths of the oily matter. Now, as theſe matters are nearly in equal proportions in bile itſelf, it muſt be admitted that ſoda contributes towards the complete diſſolution of the oil; nevertheleſs acids ſcarcely, if at all, affect it.

Bile conſiſts of a little ſoda and much oily and ſaccharine matter, &c.

In reflecting on the above experiment and its reſults, I concluded that bile was a triple compound of a little ſoda and much oily and ſaccharine matter; that acids decompoſed it but in part; in other words, that it was capable of containing an exceſs of acid without having its portion of ſoda neutralized. I therefore calcined bile that had been acidulated with ſulphuric, muriatic, and other acids, and found in each caſe the ſoda left in the calx: it is therefore very probable that the ſaccharine matter, in conjunction with the oil, decompoſed a certain quantity of marine ſalt, and deſtroyed the power of the acid.

It

Determination of component parts or bile.

It would have been of little ſervice to deſcribe the conſtituent parts of bile, had their proportions been left unaſcertained; I have therefore endeavoured to determine them in the following analyſis:

Analyſis.

By means of nitric acid, I ſeparated the animal ſubſtance, which is ſuppoſed to be albumen, with a very ſmall portion of oil: this being ſoluble in alcohol and the other not, it was eaſy to aſcertain the weight of each. I then precipitated all the oily matter, with acetite and a ſmall exceſs of oxide of lead: this precipitate being mixed with the metallic oxide, I diſſolved it in weak nitric acid; after filtering the liquor, I deprived it of the lead which remained, by means of ſulphurated hydrogen; and by evaporation, I obtained the peculiar ſubſtance, mixed, indeed with the ſalts of the bile, which had moſtly undergone a change by the acetite of lead, and whoſe weight had noted.

I aſcertained the quantity of ſoda by calcining 100 parts extracted from bile, and comparing with much care how much the reſiduum would imbibe of acid at 16°, with the quantity imbibed by pure ſoda. I alſo, by means not neceſſary to ſtate here, obtained the quantity of each of the other ſalts contained in bile; from all which experiments, made with the utmoſt care, I conclude that 800 parts of the bile of an ox contain—

Numerical reſult.

Water - - -	700 Parts
Oily matter - - -	43
Particular ſubſtance - -	41
Animal ſubſtance - -	4
Soda - - -	4
Marine ſalt - - -	3.2
Sulphate of ſoda - -	0.8
Phoſphate of ſoda - -	2
Phoſphate of lime - -	1.2
Oxide of iron - - -	0.5
	799.7

N. B. This calculation is $\frac{3}{10}$ deficient of the given quantity.

Bile forms an intereſting ſubject for a number of other reſearches: the varieties to be found in the ſeveral ſpecies of animals, and which a multitude of circumſtances, particularly a morbid affection of the organ which ſecretes it, may modify;

the calculi which are there formed, and are of a peculiar nature; the oleaginous and animal ſubſtances; and that particular matter, differing from all others hitherto known; will not fail to excite a lively intereſt, and are the ſubject of ſeveral other Papers which I purpoſe ſhortly to bring before the public.

XIV.

Quotation from Sir GEORGE STAUNTON'S *Embaſſy, containing a Deſcription of Fire Works unknown in Europe. Propoſed by a Correſpondent with a View to obtain Explanation of the Means by which they were produced.*

To Mr. NICHOLSON.

SIR,

Introduction.

I preſume to think it will accord with the general aim of your excellent collection to inſert the following quotation; and I indulge the hope that your compliance with my requeſt for that purpoſe may produce an explanation from ſome of your ingenious correſpondents.

I am Sir,

Your conſtant reader,

P. M.

Remarkable fire-works of the Chineſe.

"After the ballets, Fire-works were played off; and even in the day-time had a ſtriking effect. Some of the contrivances were new to the Engliſh ſpectators. Out of a large box, among other inſtances, lifted up to a conſiderable height, and the bottom falling out as if it were by accident, came down a multitude of paper lanterns, folded flat as they iſſued from the box, but unfolding themſelves from one another by degrees. As each lantern aſſumed a regular form, a light was ſuddenly perceived of a beautifully coloured flame, burning brightly within it; leaving doubtful by what deluſion of the ſight thoſe lanterns appeared, or by what property of combuſtible materials they became thus lighted, without any communication from the outſide to produce the flame within. This devolution and developement were ſeveral times repeated, with a difference

of figure every time, as well as of the colours, with which the Chineſe ſeem to have the art of clothing fire at pleaſure. On each ſide of the large box, was a correſpondence of ſmaller boxes, which opened in like manner, and let down a kind of net work of fire, with diviſions of various forms, which ſhone like burniſhed copper, and flaſhed like lightning at every impulſe of the wind. The whole ended with a volcano, or eruption of artificial fire, in the grandeſt ſtile."—*See Staunton's Embaſſy to China, Volume* III. *page* 73.

XV.

On the Carbonate of Potaſh. By M. STEINACHER *.

Carbonate of potaſh requires to be formed by paſſing the gas through a cold alkaline ſolution till it cryſtallizes, and not by evaporation.

CHEMISTS know that carbonate of potaſh well ſaturated, ſo as to efflureſce, can only be formed by making paſs through a cold alcaline ſolution a quantity of carbonic acid ſufficient to cauſe a ſpontaneous cryſtallization. For on ſtopping the diſengagement of gas at the moment when the earth of the alkali appears to be depoſited, and evaporating the liquor by a mild heat, there are only laminated cryſtals obtained, which ſoon deliqueſce.

The gentle heat of Curaudeau does not ſucceed.

Alkaline ley warmed by heat of tan, according to the method of M. Curaudeau, does not ſucceed any better in forming a well ſaturated alkali by evaporation. I have experienced that its cryſtals grow moiſt, and the author himſelf acknowledges a ſlight deliqueſcence.

Welter's apparatus is too complicated; and ſo is that of Pelletier.

It is generally agreed, that the apparatus of Welter, deſcribed in the twenty-ſeventh Volume of the Annales de Chimie, is too complicated, and that of M. Pelletier has been adopted in its place in almoſt all laboratories, with ſome alterations in the diſpoſition of the firſt bottle, to which is fixed a tube with a double or triple perpendicular curvature, for the introduction of the acid, or a long pipe of glaſs terminated in a tunnel. The chalk mixed with water to the conſiſtence of thin ſoup, is poured by degrees into this pipe, which is ſtopped by a glaſs rod accordingly, and the gas is forced to traverſe the bottles containing the alkaline ley.

* From the Annales de Chimie, Tom. LV.

This

This method is in my opinion attended with much inconvenience, for when a tube with many perpendicular flexures is uſed, it muſt be charged with a column of the fluid ſufficient to counterbalance the preſſure of the carbonic acid gas, and conſequently to give an elevation which expoſes it to be eaſily broken; and when a long horizontal pipe of glaſs is employed, it often happens that the chalk is exploded into the air, when the piſton is opened. The reaſon ſtated.

Another method appears to be more ſimple and commodious than this, which is ſomething like that of M. Brugnatelli, but the Italian chemiſt has not publiſhed the details of his method, without which it is impoſſible to be followed. Simpler method not yet deſcribed.

A kilogram of chalk in powder is to be put into a bottle with two or three necks, capable of containing 12 kilograms of water; on this is to be poured a litre of a mixture of one and a half kilogram of vitriolic acid, with nine kilograms of ſpring water: the gas is expelled, and a cruſt of ſulphate is formed at the ſurface of the calcareous carbonate. At the end of two hours all the reſt of the acid water is to be added, and the bottle ſtopped quickly. Bubbles of the gas will be rapidly diſengaged, but they will by degrees be diſcharged more ſlowly, and continue to be ſo moderately for twenty-four hours; then the mixture is to be ſtirred with an iron rod, and the gas will continue to be developed for 24 hours more, with little interruption.

I found the term of the effervеſcence prolonged by the reſiſtance which was oppoſed to the action of the ſulphuric acid on the chalk by the denſity which the combination neceſſarily acquired; which denſity the tendency of the ſulphuric acid to augment the ſolubility of the ſulphate of lime, fixed to proper limits.

As my apparatus, with the exception of the firſt bottle, is the ſame of that of M. Pelletier deſcribed in the fifteenth Volume of the Annales, I ſhall not ſpeak of it, but only make the following remarks:

1. Pelletier was wrong in neglecting to uſe an intermediate bottle half filled with water; which is very neceſſary to ſeparate the ſulphuric acid which the gas always brings over.

2. The tubes of an inch diameter, being too difficult to be bent, may be replaced by others of ſeven or eight lines aperture, which will do equally well.

3. The alkaline ſolution, made by two lb. alkali to three lb. water, cryſtallizes too quick, and before the precipitation of the ſilex.

4. The doſes of alkali and water moſt favourable to a regular cryſtallization, at the temperature of from 5 to 10 above 0 of Reaumur, are one part of diſtilled water, and half a part of purified potaſh.

If the reſults of my experiments ſhall improve the preparation of carbonate of potaſh, and if the diſpoſition of my apparatus prevents the neceſſity of continually watching its direction, by procuring without trouble that gentle and continued preſſure, of which Pelletier perceived the efficacy for the ſaturation of the alkali, I have reaſon to think that the true friends of chemiſtry, I mean thoſe who practiſe it, will conſider my obſervations with indulgence.

XVI.

*Method of preparing a luminous Bottle, which long preſerves its Effect.**

A luminous bottle. Put a ſmall piece of phoſphorus in a long phial: pour on it boiling oil. The fluid will give light in the vacuous ſpace whenever the cork is pulled out.

IT is eaſy to prepare a bottle which ſhall give ſufficient light during the night to admit of the hour being eaſily ſeen on the dial of a watch, as well as other objects, by the following means.

A phial of clear white glaſs, of a long form, ſhould be choſen, and ſome fine olive oil ſhould be heated to ebullition in another veſſel: A bit of phoſphorus, of the ſize of a pea, ſhould be thrown into the phial, and the boiling oil ſhould then be carefully poured over it, till the phial is one-third filled: The phial ſhould then be carefully corked; and, when it is to be uſed, it ſhould be unſtopped, to admit the external air, and cloſed again: The empty ſpace of the phial will then appear luminous, and give as much light as a dull ordinary lamp. Each time that the light diſappears, on removing the ſtopper it will inſtantly re-appear. It is proper to obſerve, that in cold weather it will be neceſſary to warm the bottle for a little while in the hands before the ſtopper is removed, without which precaution it would not yield any light.

* Sonini's Journal.

A phial

A phial thus prepared may be uſed every night for ſix months: there is no danger of fire from it, and its coſt is very ſmall.

XVII.

Analyſis of the Magneſian Earth of Baudiſſero in Canavais (in the Department of the Loire,) known by the Name of Porcelain Earth, and hitherto conſidered as a Clay. By M. GIOBERT.*

THE earth of Baudiſſero, known by the name of porcelain earth, has been hitherto conſidered as one of the pureſt argillaceous earths known in the hiſtory of foſſils, and is arranged in our cabinets of minerals as native alumine.

Porcelain earth conſidered as native alumine.

In a manufacture of ſtone-ware pottery, which has been eſtabliſhed at Vineuf, this earth has been uſed for a long time, as a clay of extraordinary purity. The celebrated Macquer, and with him Baume, to whom ſpecimens of this earth were ſent from the above manufacture, pronounces poſitively that it was a clay of ſuperior quality to that which they uſed in the manufactory of porcelain at Sevres.

Uſed as ſuch in the ſtone-ware pottery at Vineuf.

Various chemiſts adopted the ſame concluſion.

Doctor Gioanetti continued to uſe it in the manufacture of his fine porcelain at the ſame Vineuf; and he engaged in, if not an analyſis, at leaſt ſome experiments on this earth, to aſcertain more preciſely the proportions in it between ſilex and earth, which he believed to be pure alumen. Theſe experiments convinced Doctor Gioanetti, that, with the exception of a little carbonic acid which he found in it, the earth of Baudiſſero was an alumen almoſt perfectly pure, or at leaſt the pureſt that he had ever met with.

This chemiſt, when I made enquiries of him relative to this earth, aſſured me frequently, that picked pieces yielded him ſometimes ninety *per cent.* of alumen, including a little carbonic acid, and that in the groſs it yielded conſtantly at leaſt 80.

The alumine was eſtimated at 80 per cent.

On the peruſal of the mineralogical deſcription of the mountains of Canavais by the Chevalier Napion, it will be found that this eſtimable mineralogiſt has not heſitated to declare the

Other authority.

* Journal de Phyſique, LX.

earth

earth of Baudiſſero to be the moſt pure alumen ever found in Piedmont; and again in his elements of mineralogy, he mentions the earth of Baudiſſero as native alumen.

Contrary to theſe aſſertions this native earth contains no alumine at all.

Facts ſo poſitively aſſerted by ſcientific men ſo eſtimable as Maquer, Baume, and our colleagues Gioanetti and Napion, admitted no doubt of the nature of this earth; to which authorities might be added the ſucceſs with which Gioanetti conſtantly uſed it in his porcelain manufacture.

Among a number of reſearches which I made relative to the artificial fabrication of ſulphate of alumen, I employed myſelf on this earth, and to my great ſurprize found that the earth of Baudiſſero not only was not pure alumen, but did not even contain an atom of it.

Immenſe quantities of ſulphuret of iron at Baudiſſero.

The town of Baudiſſero is ſituated at leaſt three leagues from Ivree and from Brozo, this laſt village, as celebrated for its iron mines as for the manner in which they are wrought, contains in a mountain, among other minerals, an inexhauſtible maſs of ſulphuret of iron of a remarkable purity, where there is eſtabliſhed a manufacture of ſulphate of iron by the combuſtion of the ſulphur.

Efflorescence of the neighbouring blocks of ſtone by ſulphureous vapours.

On inſpecting this manufacture laſt year, I was ſtruck with the ſingularly powerful action, which the ſulphureous acid, formed by the combuſtion of the ſulphur, (and of which a part expanded itſelf to neighbouring places,) exerciſed on the great blocks of ſtone.

Theſe ſtones were a ſort of granite ſchiſtus; and the ſulphureous acid attacked them ſo forcibly that it made them exfoliate, and at laſt reduced them to a ſpecies of efflorescence, or white powder evidently ſaline, in which its aſtringent taſte announced ſulphate of alumen.

Probability that ſulphate of alumine or alum might be advantageouſly made from the porcelain earth.

This circumſtance made me think that if a good argil was expoſed to the action of the acid it would be alumenated; and the earth of Baudiſſero, which I believed to be almoſt pure alumen, being at ſuch an inconſiderable diſtance, made me conceive the hope of being able to eſtabliſh with economy at Piedmont, a manufacture of artificial ſulphate of alumen.

Very promiſing local advantages.

The idea of this eſtabliſhment appeared to me to be ſo much the more fortunate as there was at the foot of the ſame mountain which contained the pyrites, a great turbary, which extended almoſt as far as Chinſella, that is to ſay, almoſt to Baudiſſero, and which might furniſh fuel at a very ſmall expenſe;

penſe; and it ſeemed to me that nature, in placing at one ſide an inexhauſtible mine of ſulphur, and at the other inexhauſtible maſſes of the proper earth of an extremely rare purity, and between them an abundant ſupply of fuel, of the ſort moſt proper for this kind of work, had done its utmoſt in favour of the eſtabliſhment I intended.

Experiments preparatory.

There only remained to make ſome experiments with a view to aſcertain the moſt profitable way of proceeding; and to examine principally if the iron which is united to the ſulphur in the pyrites, would not be injurious to the ſulphate of alumen obtained.

With this deſign I began by examining the action of the earth of Baudiſſero on the ſulphate of iron, and the quantity of the earth neceſſary for the decompoſition of a given weight of ſulphur.

The earth of Baudiſſero decompoſed ſulphate of iron in the humid way. A little potaſh was added and the liquor ſet to cryſtallize.

In the different experiments the ſulphate of iron diſſolved in water, and boiled with this earth in different proportions became evidently decompoſed after boiling for leſs than a quarter of an hour; the iron was precipitated of a blackiſh grey, while the ſolution was colourleſs, and ammonia dropped into it formed only a very white precipitate, which did not announce much iron: I filtered the liquor, of which one part was mixed with a little potaſh, and placed ſo as to cryſtalize; and to aſcertain whether there was any potaſh in the earth of Baudiſſero, I ſet another part to cryſtalize without any alkali.

It gave ſulphate of magneſia and not allum.

I obſerved that the liquors cryſtallized immediately after becoming cold; but in the place of octahedrons, I found the moſt perfect, the moſt beautiful, and pureſt cryſtals of ſulphate of magneſia.

All the cryſtals were of this kind, and hence magneſia is an excellent porcelain earth.

The liquor which remained produced, on a new evaporation, the ſame pure cryſtals of ſulphate of magneſia; and did the ſame on ſucceſſive evaporations and cryſtalizations to the laſt drop of the liquor. In this manner was a natural alumen transformed entirely into magneſia, and at the ſame inſtant magneſia became at once an excellent porcelain earth. If examples of this kind ſhould multiply, the neceſſity of chemical analyſis for the knowledge of foſſils will become more and more manifeſt, and leſs reliance will be learned to be placed on their external and phyſical characters, which at preſent ſeems to me to be too much abuſed.

The

More careful examination of the native earth.

The above unexpected results engaged me to make a more careful examination of the earth of Baudiſſero, and which is the object of this memoir.

At the time when I found that the ſuppoſed alumen of Baudiſſero in Canavais, was really a magneſian earth, I knew of no other example of an earth truly magneſian, but that of the earth of Salinelle, or of Sommieres, which Berard had made known (*Annales de Chimie, Tome* XXXIX. p. 65.)

Other ſpecimens of native magneſia.

In this magneſian earth there is no mixture of any other earth except ſilex, and that in a very ſmall proportion, of which fact there are but few examples. But on receiving the twelfth volume of Brochant's Mineralogy, I found that the diſcovery of a magneſian earth was announced in it, which is the native carbonate of magneſia found by Doctor Mitchel at Roubſchitz in Moravia. From the analyſis which he made of it, and which is mentioned by Brochant, we are aſſured that the native carbonate of magneſia of Moravia is compoſed only of magneſia and carbonic acid in almoſt equal parts; but the yellowiſh grey colour ſpotted with black, which Doctor Mitchel gives to this earth, ſeems ſufficiently to indicate the exiſtence of ſome other conſtituent parts. On comparing the characters and nature of the magneſian earth of Baudiſſero, it will be eaſy to perceive the differences which diſtinguiſh it from the other preceding magneſian earths.

Local ſituation of the earth of Baudiſſero.

The magneſian earth of Baudiſſero is found diſpoſed in a vein in a ſteatite rock, of which the mountain is compoſed that encloſes it. It is accompanied by an horn-ſtone, ſometimes of a tranſparent pale colour, ſometimes, when its decompoſition commences, of a white almoſt opaque. In this ſtate the horn-ſtone does not appear to be that of which Doctor Bonvoiſin has given the deſcription and analyſis, under the name of the Hydrophane of Piedmont.

It is found in maſſes, lumps, or fragments.

Our magneſian earth appears in maſſes, ſometimes in roundiſh lumps (mamelonnés) and ſometimes in fragments more or leſs large; the lumps and fragments are ſometimes, but rarely, tuberculoſe.

Beautifully white.

This earth is of the moſt beautiful white, in which it differs from that of Moravia, of which the colour is a yellowiſh grey ſpotted with black, and from that of Salinelle, or of Sommigres, which is of a chocolate colour.

The

The hardneſs of this earth is variable, ſometimes it is ſoft, in which ſtate I ſhall call it the *earthy* ſort, and ſome pieces of it have a conſiderable hardneſs; as in all my experiments I tried them comparatively, I ſhall name this laſt variety the *ſtony* kind, to diſtinguiſh it from the preceding.

Soft or hard ſo much as to ſcratch ſteel.

The ſtony variety is ſcratched by ſteel, ſometimes, on the contrary, it is hard enough to ſcratch ſteel. It can be eaſily reduced to powder; but with much difficulty to very fine powder, and this only takes place after long trituration in a mortar of porphyry. Its hardneſs neither increaſes nor diminiſhes by the action of the air; in this reſpect it differs from the magneſia of Moravia, which is very friable, and from that of Salinelle, which is ſoft in its bed, and only grows hard on exſiccation in the air.

Pulverable, tho' not finely, permanent in the air.

The fracture of this variety is conchoidal and unequal.

Fracture conchoidal.

Its ſurface is dull; ſometimes, but very rarely, ſhining ſpots appear. It is conſtantly perfectly opaque, and moderately heavy; its ſpecific gravity is variable.

Dull, opake, moderately heavy.

It is a little unctuous to the touch in the friable and earthy ſort, and but very little ſo in the ſtony variety.

Slightly unctuous and adheſive.

It ſenſibly adheres to the tongue, though not much; it acquires this property in a conſiderable degree when it is moderately warmed at the fire.

Plunged in water, the ſtony variety does not abſorb it at all; the friable ſort abſorbs it greedily, and with an hiſſing, but the mixture does not grow hot.

The ſoft ſpecimens abſorb water and mix like clay.

The friable ſpecies mixes with water to a conſiderable degree, in the ſame manner as clay; the fine particles of this earth, like thoſe of clay, continue a long time ſuſpended in water, with this difference from thoſe of clay, that they do not unite together. Urged by the blow-pipe, on a cianite cryſtal, it is infuſible.

Are not fuſible by the blow-pipe.

Treated in a maſs, on the fire in a crucible, eſpecially in a red hot crucible, it ſoon decrepitates, and divides into thick ſcaly pieces, which leap out of the crucible; this does not happen if it is heated by degrees and moderately.

If it is reduced to a fine powder, and then traited on the fire, as ſoon as the bottom of the crucible begins to grow red hot, this earth boils for a ſhort time, and ſeems to unite together, as if moderately moiſtened.

But apparently ſo in a crucible.

An

With loſs of one ſeventh.

An hundred parts of this earth treated in this manner, until the boiling ceaſes, after an hour of incondeſcence, became reduced to 85, and 0,40. The earth calcined in this manner throws out that blueiſh light which has been obſerved from common magneſia.

Giving out carbonic acid.

If the calcination is made in a retort of earthen ware, to which a ſyphon is adapted, and plunged into a bottle filled with lime water, there is formed in the bottle carbonate of lime; ſo that the diminution of weight is partly due to the diſengagement of carbonic acid.

It contains a minute quantity of ſulphate of lime.

A thouſand grains of this earth in fine powder were boiled in ſix pounds of diſtilled water. The liquor being filtered, and then eſſayed by various reagents, preſented the following reſults.

With the ſolutions of the acetate, nitrate, and muriate of barytes, the mixture became troubled almoſt inſtantly, and formed a ſediment of ſulphate of barytes, but in a very ſmall quantity.

The oxalate of ammonia formed oxalate of lime with it, but alſo in a very ſmall portion.

Theſe experiments repeated different times on the earth, both of the ſtony and friable varieties, conſtantly gave the ſame reſults.

Lime and ſulphuric acid, or ſulphate of lime, is therefore, although in a ſmall degree, a conſtituent part of the earth of Baudiſſero both in the ſtony and ſoft ſtate.

A minute portion of muriatic acid ſeems to be preſent in the ſtony variety.

The nitrate of ſilver formed a precipitate equally with both ſorts; but remarkable differences were obſerved between its effects in the water from the ſtony ſpecies, and on that from the ſoft variety; with the latter it formed a precipitate, which collected in a powder at the bottom of the glaſs; but with the water from the ſtony kind, beſides the precipitate, filaments were produced conſtantly, which indicated the preſence of muriatic acid. Many times the "ſulphuric acid*" was firſt removed by the acetate of barytes, and after filtration, on being treated with the nitrate of ſilver, ſtill formed a precipitate of muriate of ſilver.

* It is not clear what the removal of the *ſulphuric acid* mentioned here had to do with making the appearance of the *muriate of ſilver* ſeem extraordinary; perhaps it is an error in the original; the tranſlation is literal and correct. B.

The infuſion of the ſtony ſort afforded differences from the other, with ammonia alſo; this reagent did not ever trouble the infuſion of the friable ſpecies, but always troubled, though ſlightly, the infuſion of the ſtony variety.

It follows from theſe obſervations that, beſides the ſulphate of lime which both kinds of the earth of Baudiſſero contained, the ſtony variety held in union muriatic acid, perhaps in combination, partly with the lime, which there was not ſufficient ſulphuric acid to ſaturate, and partly united to another earth, which was not lime, ſince its ſolution permitted itſelf to be decompoſed by ammonia; and it will appear that this earth was magneſia.

The ancient mineral acids attack this native earth.

The ſulphuric, nitric, and muriatic acids attack this earth, when it is well divided into an extremely fine powder.

Their action however is but little apparent, but on the leaſt addition of heat it becomes ſtrongly marked. Very ſmall bubbles of gas, which riſe from the bottom of the liquor, a ſlight white ſcum which forms itſelf at the ſurface, and a ſmall hiſſing, ſhews plainly that there is a diſengagement of an aeriform fluid or effervеſcence.

With great force if previouſly calcined.

When the earth has been previouſly calcined in the fire, their action is very different. There is not, as may be foreſeen, any effervеſcence; but the mixture grows hot, to that degree that a true ebullition enſues; in ſome minutes the mixture aſſumes a ſolid form, cauſed by a kind of jelly produced by it.

The muriatic acid acts more ſtrongly.

The acid which has the greateſt action on it is the muriatic acid, and after this the nitric, and the ſulphuric acid after both. This laſt however does not diſſolve without much difficulty the whole of the ſoluble part, and that after a long continued ebullition.

The ſolution made in the cloſed veſſels diſpoſed ſo as that the gas may be received, forms with lime-water carbonate of lime, which confirms the diſengagement of a little carbonic acid before indicated by the calcination of this earth in the fire.

The ſolutions of this earth in the acids are perfectly colourleſs.

The pruſſiate of lime or the oxalate of ammonia do not at all trouble them.

Ammonia forms with them an abundant precipitate.

Ammonia precipitates the ſolution as does likewiſe the carbonate of potaſh.

The

The common unſaturated carbonate of potaſh forms alſo a precipitate with them.

When this carbonate ceaſes to trouble the liquor, and that, after being left to ſettle and being filtered, the clear liquor is ſubmitted to ebullition, it becomes troubled again and throws down a ſecond time an earthy precipitate.

But not if ſaturated with c. acid.

Finally, if inſtead of the unſaturated carbonate, the carbonate of potaſh, well ſaturated with acid is uſed, there is not the leaſt precipitate formed.

The earth is pure magneſia.

Experiments, which I will relate, ſhew not only that the earth diſſolved by the acids is magneſia, but that there is not mixed with it the leaſt particle of lime, which can be diſcovered by the oxalate of ammonia, that there is no trace of alumen in it, which the ſaturated carbonate of potaſh precipitates, and does not again diſſolve; that it does not contain the leaſt oxide of iron, that can be indicated by the pruſſiate of lime; and finally, that it is magneſia perfectly pure.

As is ſhewn by the ſulphate.

This reſult is farther confirmed by the ſulphate of magneſia, which the cryſtallization of the ſolution of this earth in ſulphuric acid yields excluſively.

Inſoluble reſidue or ſilex about one 6th.

The acids in diſſolving this earth leaves a reſidue, the quantity of which ſeems to be variable; that which the ſulphuric acid leaves is conſtantly more than what is left by the muriatic or nitric acids. An hundred and twenty grains of this earth, after being well lixiviated in pure water, left a reſidue of which the weight, in the different experiments which I made, did not ever exceed 17 grains, and never was leſs than 14. The ſtony variety was that which in general gave the moſt of this inſoluble reſidue. Many experiments, which I have made, and which it would be uſeleſs to repeat here, have convinced me that this reſidue is perfectly pure ſilex.

Component parts recapitulated.

The earth of Baudiſſero, from the preceding experiments, conſiſts entirely of magneſia with a little carbonic acid, a ſmall quantity of ſilex, and a very minute portion of ſulphate of lime, with, in the ſtony variety, ſome traces of muriate of magneſia.

(To be concluded in the Supplement.)

XVIII.

Experiments for aſcertaining how far Teleſcopes will enable us to determine very ſmall Angles, and to diſtinguiſh the real from the ſpurious Diameters of celeſtial and terreſtrial Objects: with an Application of the Reſult of theſe Experiments to a Series of Obſervations on the Nature and Magnitude of Mr. HARDING'S *lately diſcovered Star. By* WILLIAM HERSCHEL, *L. L. D. F. R. S. Abridged from the Phil. Tranſ.* 1805.

Enquiry as to the ſmalleſt angle under which the eye by a teleſcope can determine the figure of an object.

THE diſcovery of Mr. Harding having added a moving celeſtial body to the liſt of thoſe that were known before, Dr. Herſchel was deſirous of aſcertaining its magnitude: and as in the obſervations which it was neceſſary to make he intended chiefly to uſe a ten-feet reflector, it appeared to be a deſideratum highly worthy of inveſtigation to determine how ſmall a diameter of an object might be ſeen by this inſtrument. It is known that a very thin line may be perceived, and that objects may be ſeen when they ſubtend a very ſmall angle; but the caſe he wanted to determine related to a viſible diſk; a round, well defined appearance, which might without heſitation be affirmed to be circular, if not ſpherical.

The author's unaſſiſted eye cannot diſtinguiſh a ſmaller circle than of 2′ 17″ from an equal ſquare.

In April of the year 1774, the Doctor determined a ſimilar queſtion relating to the natural eye: and found that a ſquare area could not be diſtinguiſhed from an equal circular one till the diameter of the latter came to ſubtend an angle of 2′ 17″.

He did not think it right to apply the ſame concluſions to a teleſcopic view of an object, and therefore had recourſe to the a ſeries of experiments.

Experiments with the teleſcope: the objects were pin's heads.

The firſt courſe of experiment, was made with the heads of pins deprived of their poliſh by tarniſhing them in the flame of a candle. The diameters of the heads were meaſured by a microſcopic projection, with a magnifying power of 80. Theſe meaſures were ſo exact, than when repeated they ſeldom differed more than a few ten thouſandths part of an inch from each other. The focal length of the mirror on Arcturus is 119,64 inches, but on theſe objects 125,9, and the diſtance was meaſured with deal rods.

And the reſult of this experiment was that an object having a diameter ,0425 could be eaſily ſeen in the author's ten-foot teleſcope to be a round body, when the magnified angle under which

which it appeared was 2′ 18″,9, and that with a high power (522) a part of it, ſubtending an angle of 0″,364 may be conveniently perceived.

When the purpoſe of this experiment, was conſidered, the reſult was not found ſufficient to anſwer the intention; for as the ſize of the object required the uſe of a low power, a doubt aroſe whether the inſtrument would be equally diſtinct when a higher ſhould be required. To reſolve this queſtion, it was neceſſary either to remove my objects to a greater diſtance, or to make them ſmaller.

With globules of ſealing-wax.

2. Small globules of ſealing-wax were therefore made by dipping the point of a fine needle, a little heated, into it, which took up a ſmall globule. To prevent ſeeing them at a diſtance in a different aſpect from that in which they were meaſured under the microſcope, the needles were fixed with ſealing-wax on ſmall ſlips of cards before the meaſures were taken.

By this experiment it was found, that with a globule ſo ſmall as ,00763 of a ſubſtance not reflecting much light, the magnified angle muſt be between 4 and 5 minutes before we can ſee it round. But it alſo ſhewed that a teleſcope with a ſufficient power (522) will ſhow the diſk of a faint object when the angle it ſubtends as the neked eye is no more than 0″,653.

Globules of ſilver.

The third experiment was made with globules of ſilver. As the objects made of ſealing-wax, on account of their colour, did not appear to be fairly ſelected for theſe inveſtigations, a ſet of ſilver ones were made. They were formed by running end of ſilver wires, the 305th and 340th part of an inch diameter, into the flame of a candle.

By this experiment it was found that the teleſcope acted very well with a high power (522), and will ſhow an object ſubtending only 0″,484 ſo large that it may be divided into quarters of its diameter,

Experiments with other globules, and with ſilver at greater diſtances, &c.

The fourth experiment was made with globules of pitch, bee's-wax, and brimſtone, and did not prove ſo generally advantageous as thoſe with ſilver which reflected more light.

And a fifth and ſixth experiment was made with the ſilver globules at greater diſtances; and by illuminations at night by the flame of a lamp of which for brevity the particulars are here omitted.

The

The author then proceeded to make direct obſervations on the ſpurious diameters of celeſtial bodies, from which he deduces the following reſults:*

(1.) As the diameters of fixed ſtars are undoubtedly ſpurious, it follows that, with the ſtars, the ſpurious diameters are larger than the real ones, which are too ſmall to be ſeen. Spurious diameters of ſtars greater than real.

(2.) From many eſtimations of the ſpurious diameters of the ſtars † it follows, not only that they are of different ſizes, but alſo that under the ſame circumſtances, their dimenſions are of a permanent nature. Sizes are the ſame in like circumſtances;

(3.) By this and many other obſervations it appears, that the ſpurious diameters of the ſtars are differently coloured, and that theſe colours are permanent when circumſtances are the ſame. and colours.

(4.) By many obſervations, a number of inſtances of which may be ſeen in Dr. Herſchel's catalogues of double ſtars, their ſpurious diameters are leſſened by increaſing the magnifying power, and increaſe when the power is lowered. They are leſs with high powers;

(5.) It is alſo proved by the ſame obſervations, that the increaſe and decreaſe of the ſpurious diameters, is not inverſely as the increaſe and decreaſe of the magnifying power, but in a much leſs ratio. but not proportionally.

(6.) The magnifying power acts unequally on ſpurious diameters of different magnitudes; leſs on the large diameters, and more on the ſmall ones. Small ſtars are moſt enlarged.

(7.) When the aperture of the teleſcope is leſſened, it will occaſion an increaſe of the ſpurious diameters, and when increaſed will reduce them. Leſs aperture cauſes greater ſp. diameter;

(8.) It alſo ſhows that the increaſe and decreaſe of the unequal ſpurious diameters, by an alteration of the aperture of the teleſcope, is not proportional to the diameters of the ſtars: and ſmall ſtars are moſt affected by this change.

(9.) But that this alteration acts more upon ſmall ſpurious diameters, and leſs upon large ones.

(10.) From this we find that ſtars, when they are extremely ſmall, loſe their ſpurious diameters, and become nebulous. Very ſmall ſtars appear nebulous.

* On this ſubject ſee our Journal, Vol. VI. p. 15, and Fig. 1, Plate IV.

† See Catalogue of double Stars. Phil. Tranſ. for 1782, p. 115; and for 1785, p. 40.

(11.) Many

Other cauſes affect the ſpurious diameters.

(11.) Many other cauſes will have an influence on the apparent diameter of the ſpurious diſks of the ſtars, ſuch as the goodneſs of the ſpecula and lenſes; but they are ſo far within the reach of our knowledge, that with a proper regard to them, the concluſion he has drawn in Rem. (2.) "that under the ſame circumſtances their dimenſions are permanent," will ſtill remain good.

Similar experiments were made on the ſpurious diameters of terreſtrial objects, namely ſilver globules, which afforded the following reſults:

Spurious diſks of globules are ſmaller than the real diſks.

(1.) The terreſtrial ſpurious diſks of globules are leſs than the real diſks; whereas we have ſeen, in Remark (1.) of the celeſtial ſpurious diſks, that theſe are larger than the real ones.*

Larger magnitudes give larger ſp. diſks;

(2.) The leſs globule has the largeſt ſpurious diſk. This agrees with the ſpurious diſks of celeſtial objects: the ſtars of the firſt, ſecond, and third magnitude, having a larger ſpurious diſk than thoſe that are of inferior magnitudes.

coloured like the celeſtial:

(3.) With reſpect to colours, the terreſtrial alſo agree with the celeſtial ſpurious diſks.

Leſs with greater mag. power;

(4.) The ſpurious diameters of the globules, like the ſpurious diſks of the ſtars, are proportionally leſſened by increaſing the magnifying power, and increaſed when the power is lowered.

But not proportionally.

(5.) When the eſtimations are compared with the powers, it will alſo be ſeen that the increaſe and decreaſe of the ſpurious diſks of the globules is not inverſely as the powers, but in a much leſs ratio.

Power acts more on ſmall than large ſp. diſks,

(6.) The effect of magnifying power is unequally exerted on ſpurious diameters; and that, as with celeſtial objects, ſo with terreſtrial, this power acts more on the ſmall ſpurious diſks than on the large ones.

and diminution of aperture;

(7.) The ſpurious terreſtrial diſks alſo reſemble thoſe of the ſtars, by increaſing when the aperture is leſſened, and decreaſing when it is enlarged.

greater on ſmall diſks.

(8.) By theſe experiments it is proved, that the increaſe and decreaſe of the diameters occaſioned by different apertures is not proportional to the diameters of the ſpurious diſks.

* It appears from the context, that this ariſes from the terreſtrial ſpurious diſks being formed by the ſmall ſpot of reflected light from the metallic globule, and not from its whole diameter.

(9.) But that the change of the apertures acts more on the ſmall, and leſs on the large ones.

(10.) The ſpurious diſks of globules are loſt for want of proper illumination, but do not change their magnitude on that account. The brightneſs of the atmoſphere in a fine day is ſufficient to produce them; though the illumination of the ſun is generally the principal cauſe of them. These diſks are not changed by diminution of light.

(11.) The diameters of ſpurious diſks are liable to change from various cauſes; an alteration in the direction of the illumination will make the reflection come from a different part of the globule, which can hardly be expected to be equally poliſhed in its ſurface, or of equal convexity every where, being very ſeldom perfectly ſpherical; but as upon the whole the figure of them is pretty regular, the apparent diameter of the ſpurious diſks will generally return to its former ſize.

Globules of mercury were uſed inſtead of thoſe of ſilver, and with the ſame reſults. Mercurial globules.

The ſpurious terreſtrial diſks were then meaſured by comparing them with circles on a tablet: and it was found that a variation in their illumination did not affect their magnitude. It was alſo found that the rays from the central part of the mirror gave a larger image than thoſe from its circumference. So that when a central aperture of three inches gave an image correſponding with a circle of 0.465 inch, an annular opening from 6.5 to 8.8 inches gave only 0.18 inch for the image: and the experiments were ſufficiently varied as to the magnitudes and ſituations of the apertures to ſhew that this difference did not ariſe from more or leſs light. Meaſurement of ſpurious diſks. They may be diſtinguiſhed from real diſks by uſing firſt a central, and then an annular aperture. The firſt enlarges and the ſecond diminiſhes them.

This property of the mirror ſerves admirably to diſtinguiſh a ſpurious diſk from a real one; and proved to be ſo on trial with terreſtrial and celeſtial objects. Trials.

Obſervations on the Nature and Magnitude of Mr. HARDING'S *lately diſcovered Star.*

On the day Dr. Herſchel received an account of Mr. Harding's new ſtar, which was the 24th of September, he directed his teleſcope to the calculated place of the new object, and noted all the ſmall ſtars within a limited compaſs about it. They were then examined with a diſtinct high magnifying power; and ſince no difference in their appearance Obſervations on the planet Juno.

Obſervations on the planet Juno.

was perceivable, it became neceſſary to attend to the changes that might happen in the ſituation of any one of them. They were delineated as in Fig. 1, (Plate XIV.) which is a mere eye-draught, to ſerve as an elucidation to a deſcription given with it in the journal; and the ſtar marked *k*, was the new object.

Sept. 29. Being the firſt clear night, he began a regular ſeries of obſervations: and as the power of determining ſmall angles, and diſtinctneſs in ſhowing minute diſks, whether ſpurious or real, of the inſtrument he uſed on this occaſion, had been ſufficiently inveſtigated by the foregoing experiments, there could be no difficulty in the obſervation, with reſources that were then ſo well underſtood, and have now been ſo fully aſcertained.

"Mr. Harding's new celeſtial body precedes the very ſmall ſtar in Fig. 3, between 29 and 33 Piſcium, and is a little larger than that ſtar; it is marked A. *f g h* are taken from Fig. 1. I ſuppoſe *g* to be of about the 9th magnitude, ſo that the new ſtar may be called a ſmall one of the 8th."

With his ten-feet reflector, power 496.3, he viewed it attentively, and comparing it with *g* and *h*, Fig. 3, could find no difference in the appearance but what might be owing to its being a larger ſtar.

By way of putting this to a trial, he changed the power to 879,4, but could not find that it magnified the new one more than it did the ſtars *g* and *h*.

"I cannot perceive any diſk; its apparent magnitude with this power is greater than that of the ſtar *g*, and alſo a very little greater than that of *h*; but in the finder, and the night-glaſs g is conſiderably ſmaller than the new ſtar, and *h* is alſo a very little ſmaller."

He compared it now with a ſtar which in the finder appeared to be a very little larger; and in the teleſcope with 879,4 the apparent magnitude of this ſtar was alſo larger than that of the new one.

"As far as I can judge without ſeeing the aſteroids of Mr. Piazzi and Dr. Olbers at the ſame time with Mr. Harding's, the laſt muſt be at leaſt as ſmall as the ſmalleſt of the former, which is that of Dr. Olbers."

"The ſtar *k*, Fig. 1, obſerved Sept. 24, is wanting, and was therefore the object I was in ſearch of, which by computation muſt have been that day in the place where I ſaw it."

" The new ſtar being now in the meridian with all thoſe to which I am comparing it, and the air at this altitude being very clear, I ſtill find appearances as before deſcribed: the new object cannot be diſtinguiſhed from the ſtars by magnifying power, ſo that this celeſtial body is a true aſteroid." Obſervations on the planet Juno.

Mr. Bode's ſtars 19, 25 and 27 Ceti are marked 7m, and by comparing the aſteroid, called Juno, with theſe ſtars, it has the appearance of a ſmall one of the 8th magnitude.

With regard to the diameter of Juno, the author remarks that had it been half a ſecond, he muſt have inſtantly perceived a viſible diſk. Such a diameter, when he ſaw it magnified 879,4 times, would have appeared under an angle of 7′ 19″,7, one half of which, it will be allowed, from the experiments that have been detailed, could not have eſcaped his notice.

Oct. 1. Between flying clouds, the aſteroid was ſeen, which in its true ſtarry form has left the place where it was ſeen Sept. 29. It has taken the path in which by calculation it was expected to move. This aſcertains that no miſtake in the ſtar was made when laſt obſerved.

Oct 2, 7h. Mr. Harding's aſteroid is again removed, but is too low for high powers.

8h 30′. Viewed it now with 220,3 288,4 410,5 496,3 and 879,4. No other diſk was viſible than that ſpurious one which ſuch ſmall ſtars have, and which is not proportionally magnified by power,

With 288,4, the aſteroid had a larger ſpurious diſk than a ſtar which was a little leſs bright, and a ſmaller ſpurious diſk than another ſtar that was a little more bright.

Oct 5, with 410,5. The ſituation of the aſteroid is now as in Fig. 4. Its diſk, which is probably the ſpurious appearance of ſtars of that magnitude, was compared with a larger, an equal, and a ſmaller ſtar. It was leſs than the ſpurious diſk of the larger, equal to that of the equal, and larger than that of the ſmaller ſtar. The gradual difference between the three ſtars is exceedingly ſmall.

" With 496,3, and the air uncommonly pure and calm, I ſee ſo well that I am certain the diſk, if it be not a ſpurious one, is leſs than one of the ſmalleſt globules I ſaw this morning in the tree."

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The diameter of this globule was ,02. It ſubtended an angle of 0″,429, and was of ſealing-wax; had it been a ſilver one, it would have been ſtill more viſible.

With 879,4. All comparative magnitudes of the aſteroid and ſtars, remain as with 496,3.

The minute double ſtar *q* Ophiuchi * was ſeen in high perfection, which proves that the air is clear, and the teleſcope in good order.

The aſteroid being now in the meridian, and the air very pure, the comparative diameter ſeems a little larger than that of an equal ſtar, and its light alſo differs from ſtar-light. Its apparent magnitude, however, can hardly be equal to that of the ſmalleſt globule obſerved this morning. This globule meaſured ,01358, and at the diſtance of 9620,4 inches ſubtended an angle of 0″,214.

When the aſteroid was viewed with 879,4, more hazineſs was found than an equal ſtar would have given: but this the Doctor aſcribes to want of light. What he calls an equal ſtar, is one that in an achromatic finder appears of equal light.

Oct. 7. Mr. Harding's aſteroid has continued its retrograde motion. The weather is not clear enough to allow the uſe of high powers.

Oct. 8. If the appearance reſembling the ſpurious diſks of ſmall ſtars, which I ſee with 410,5 in Mr. Harding's aſteroid, ſhould be a real diameter, its quantity then by eſtimation may amount to about 0″,3. This judgment is founded on the facility with which I can ſee two globules often viewed for this purpoſe.

The angle of the firſt is 0″,429, and of the other 0″,214; and the aſteroid might be larger than the latter, but certainly was not equal to the former.

With 496,3, there is an ill-defined hazy appearance, but nothing that may be called a diſk viſible. When there is a glimpſe of more condenſed light to be ſeen in the centre, it is ſo ſmall that it muſt be leſs than two-tenths of a ſecond.

To decide whether this apparent condenſed light was a real or ſpurious diſk, he applied different limitations to the aperture of the teleſcope, but found that the light of the new ſtar was too feeble to permit the uſe of them. From this he concluded

* See Cat. of double Stars, I. 87.

that

that an increaſe of light might now be of great uſe, and viewed the aſteroid with a fine 10-feet mirror of 24 inches diameter, but found that nothing was gained by the change. The temperature indeed of theſe large mirrors is very ſeldom the ſame as that of the air in which they are to act, and till a perfect uniformity takes place no high powers can be uſed. Obſervations on the planet Juno.

The aſteroid in the meridian, and the night beautiful. After many repeated compariſons of equal ſtars with the aſteroid, I think it ſhows more of a diſk than they do, but it is ſo ſmall that it cannot amount to ſo much as 3-tenths of a ſecond, or at leaſt to no more.

It is accompanied with rather more nebuloſity than ſtars of the ſame ſize.

The night is ſo clear, that I cannot ſuppoſe viſion at this altitude to be leſs perfect on the ſtars, than it is on day objects at the diſtance of 800 feet in a direction almoſt horizontal.

Oct. 11. By comparing the aſteroid alternately and often with equal ſtars, its diſk, if it be a real one, cannot exceed 2, or at moſt 3-tenths of a ſecond. This eſtimation is founded on the comparative readineſs with which every fine day I have ſeen globules ſubtending ſuch angles in the ſame teleſcope, and with the ſame magnifying power.

"The aſteroid is in the meridian, and in high perfection. I perceive a well defined diſk that may amount to 2 or 3-tenths of a ſecond; but an equal ſtar ſhows exactly the ſame appearance, and has a diſk as well defined and as large as that of the aſteroid."

Reſult and Application of the Experiments and Obſervations.

We may now proceed to draw a few very uſeful concluſions from the experiments that have been given, and apply them to the obſervations of the ſtar diſcovered by Mr. Harding; and alſo to the ſimilar ſtars of Mr. Piazzi and Dr. Olbers. Theſe kind of corollaries may be expreſſed as follows.

(1.) A 10-feet reflector will ſhew the ſpurious or real diſks of celeſtial and terreſtrial objects, when their diameter is $\frac{1}{4}$ of a ſecond of a degree; and when every circumſtance is favourable, ſuch a diameter may be perceived ſo diſtinctly, that it can be divided by eſtimation into two or three parts.

(2.) A

Obſervations on the planet Juno.

(2.) A diſk of $\frac{1}{4}$ of a ſecond in diameter, whether ſpurious or real, in order to be ſeen as a round, well defined body, requires a diſtinct magnifying power of 5 or 6 hundred, and muſt be ſufficiently bright to bear that power.

(3.) A real diſk of half a ſecond in diameter will become ſo much larger by the application of a magnifying power of 5 or 6 hundred, that it will be eaſily diſtinguiſhed from an equal ſpurious one, the latter not being affected by power in the ſame proportion as the former.

(4.) The different effects of the inſide and outſide rays of a mirror, with regard to the appearance of a diſk, are a criterion that will ſhow whether it is real or ſpurious, provided its diameter is more than $\frac{1}{4}$ of a ſecond.

(5.) When diſks, either ſpurious or real, are leſs than $\frac{1}{4}$ of a ſecond in diameter, they cannot be diſtinguiſhed from each other; becauſe the magnifying power will not be ſufficient to make them appear round and well defined.

(6.) The ſame kinds of experiments are applicable to teleſcopes of different ſorts and ſizes, but will give a different reſult for the quantity which has been ſtated at $\frac{1}{4}$ of a ſecond of a degree. This will be more when the inſtrument is leſs perfect, and leſs when it is more ſo. It will alſo differ even with the ſame inſtrument, according to the clearneſs of the air, the condition, and adjuſtment of the mirrors, and the practical habits of the obſerver.

XIX.

Account of ſome new Improvements on Steam-Engines. By Mr. ARTHUR WOOLF.

Mr. Woolf's improvements in ſteam-engines.

IN our eighth volume, p. 262, we gave a ſhort account of a former improvement made by Mr. Woolf on the ſteam-engine, founded on a diſcovery that ſteam, of any higher temperature than that of boiling water, if allowed to paſs into another veſſel kept at the ſame temperature as the ſteam itſelf, will expand to as many times its volume, and ſtill be equal to the preſſure of the common atmoſphere, as the number of pounds which ſuch ſteam, before being allowed to expand, could maintain on each ſquare inch of a ſafety-valve expoſed

expoſed to the atmoſphere: for example, that maſſes or quantities of ſteam of the expanſive force of 20, 30, or 50 pounds the ſquare inch of a common ſafety-valve, will expand to 20, 30, or 50 times its volume, and ſtill be reſpectively equal to the atmoſphere, or capable of producing a ſufficient action againſt the piſton of a ſteam-engine to cauſe the ſame to riſe in the old engine (with a counterpoiſe) of Newcomen, or to be carried into the vacuous part of the cylinder in the improved engines firſt brought into effect by Meſſrs. Boulton and Watt. Mr. Woolf's improvements in ſteam-engines.

In conſequence of this diſcovery Mr. Woolf was enabled to uſe his ſteam twice (if he choſe), and with complete effect; nothing more being neceſſary than to admit high ſteam, ſuppoſe of 40 pounds the ſquare inch, into one cylinder, to work there by its expanſive force, and then to allow the ſame ſteam to paſs into, and expand itſelf in, another cylinder of forty times the ſize of the firſt, there to work by condenſation in the common way. Or with only one cylinder, by admitting a proportionally ſmall quantity of high ſteam into it from the boiler, Mr. Woolf, found that he could effect a conſiderable ſaving in fuel.

In this firſt improvement of Mr. Woolf, though the ſaving might be carried a conſiderable length, it was ſtill neceſſarily limited by the ſtrength of materials; for in the employment of high ſteam there muſt always be ſome danger of an exploſion. Mr. Woolf, however, by a happy thought, has completely obviated every danger of this kind, and can now take the full advantage of the expanſive principle without the leaſt danger whatever. This he effects by throwing into common ſteam the additional temperature neceſſary for its high expanſion, *after the ſteam is admitted into the working cylinder*, which is heated by means adequate to the end intended to be gained; and the advantage which he thus gains he effectually ſecures by a moſt ingenious improvement in the piſton. It may be eaſily conceived that ſteam of ſuch high rarity as Mr. Woolf employs, could not be made fully effective with the piſton in common uſe; for in proportion to its rarity ſo muſt be the facility with which a portion of it would eſcape, and paſs by the ſide of the piſton to the vacuous part of the cylinder: but Mr. Woolf's contrivance ſeems perfectly adapted to prevent the loſs of even the ſmalleſt portion of the ſteam.

Beſides

Beſides theſe improvements on the common ſteam-engine, he has alſo found means to apply the ſame principles to the old engine, known by the name of Savary's, in ſuch a way as to render the ſame a powerful and œconomical engine for a great variety of purpoſes.

Such is the outline of Mr. Woolf's improvements on this uſeful engine: but, for the general information of practical engineers, we ſhall ſubjoin a more technical deſcription, in Mr. Woolf's own words, extracted from his ſpecification of his patent.

(To be continued.)

SCIENTIFIC NEWS.

Geometry.

Two theorems from the Horologium of Huygens.

HUYGENS has given the two following theorems in his *Horologium Oſcillatorium*, which are applicable to all ſolid bodies: "*The center of oſcillation, and that of ſuſpenſion are always reciprocal to one another. The ſame body is always iſochronal to itſelf, while it oſcillates round parallel axis's taken at equal diſtances from the center of gravity.* M. Biot has given a remarkable extenſion to theſe two theorems.

—extended farther by M. Biot.

All theſe parallel axis's form the ſurface of a right angled cylinder of which the axis paſſes through the center of gravity. But the analytical expreſſion under which M. Biot preſents the theorem of Huygens, made him inſtantly perceive, that an arbitrary inclination might be given to this axis, the radius of the cylinder being ſuitably changed at the ſame time; and that thus according to the different degrees of inclination of the axis, an infinity of cylinders might be obtained. The ſuperficies of which cylinders ſhould have the ſame property as that of the primitive cylinder. Beſides this, the axis without changing its inclination may deſcribe a conical ſurface about its primitive poſition, which will multiply the number of cylinders already found, as often times as right lines can be conceived to be drawn on the upper ſurface of the cone.

Aſtronomy.

M. Pictet has made an obſervation of an occultation of the pleiades by the moon, on the 19th of *November*, 1804, from the Obſervatory of Geneva. M. Pictet on the occultation of the pleiades by the moon.

An account of an occultation of π ſcorpion, obſerved on the 17th of *July*, 1803, from the ſummit of Caſuleta, a mountain in the kingdom of Spain, was found among the papers of the late M. Mechain, which will appear in the 6th Volume of the Memoirs of the French National Inſtitute: this is the laſt obſervation of this kind made by a man of ſcience, whoſe premature loſs the Inſtitute will long regret. M. Mechain on the occultation of π ſcorpion,

A long ſucceſſion of obſervations was alſo found among his papers, relative to the comet which he had diſcovered from Barcelona in 1793, which will alſo appear in the ſame publication. and of the comet in 1793.

Geography.

M. Humboldt has read before the *Inſtitute Nationale, A Memoir on the Longitude of Mexico*, the capital of the kingdom ſo called. The longitude of Mexico determined accurately by M. Humboldt.

Geographers diſagree with regard to the poſition of this important point. The conſiderable difference which M. Humboldt found between his firſt obſervation, and the laſt which had been formerly made by others before him, engaged him to repeat it as often as he could, and by different methods. The diſtances of the moon from the ſtars, and ſeveral eclipſes of Jupiter's moons, always gave the ſame reſult, which is doubtleſsly preferable to all thoſe which have appeared hitherto.

Electricity.

Since the diſcovery of electrical conductors by Dr. Franklin, many philoſophers have repeated experiments to eſtabliſh the identity of electrical fire and lightning, by experiments with ſuch inſolated conductors. Conductor contrived to prevent accidents,

Theſe experiments ſucceeded to the wiſh of all who tried them; but it was ſoon perceived, that they were attended with much danger: and ſince the death of Profeſſor Richman, of Peterſburg, who was ſtruck by lightning from his conductor in 1753, few have ventured to repeat the experiment.

M. Beyer

M. Beyer of Paris has formed in his garden an apparatus of this kind, which is very ſimple, and at the ſame time perfectly effectual without any danger: It is a conductor which can alternately at pleaſure be inſolated, or not inſolated, and made to act either with a ball, or with a point. The communications between it and the earth are well eſtabliſhed, and as the obſervations can be made at more than an hundred feet from the apparatus, there is not the leaſt danger of any accident.

can be inſolated or not, at pleaſure.

Aeroſtation.

Balloon projected by Mr. Robertſon, 132 feet diameter, to carry up 50 men,

The celebrated Aeronaut Robertſon, who aſcended from Peterſburg laſt year, is endeavouring to obtain the neceſſary aſſiſtance at that place for the conſtruction of an air balloon on a very large ſcale; he propoſes that it ſhall be 732 feet in diameter, which he calculates will carry up 37 ton, and which he ſuppoſes, therefore, will eaſily ſupport 50 people, and all neceſſary accommodations for them.

and a veſſel with ſails, &c.

It is to have attached to it a veſſel furniſhed with maſts, ſails, and every other article requiſite for navigating the ſea in caſe of accidents, and provided with a cabin for the aeronauts, properly fitted up, galley for cooking, proper ſtores for ſtowing proviſions, and ſeveral other conveniencies. To render the aſcent more ſafe, it is to take up another ſmaller balloon within it, and a parachute, which will render the deſcent perfectly gentle, if the outer balloon burſts.

and an internal balloon and parachute.

From its conſtruction it will be calculated to remain in the air ſeveral weeks, in which time many experiments in natural philoſophy, and aſtronomical obſervations may be made: It is alſo ſuppoſed, that geography may be conſiderably improved by its means, as the aeronauts will be neither ſtopped in their courſe by mountains or foreſts; and ſome have even thought, that with the aſſiſtance of the trade winds, a voyage round the earth might be made in it between the tropics. Its coſt, it is calculated, will be nearly equal to that of a ſhip of the line.

Univerſity of Charkow.

The court of Peterſburg publiſhed the act of confirmation of the Univerſity of Charkow, on the 16th of May, of which the following are the chief particulars.

The

The Univerſity is under the care of the Miniſter of public inſtruction: It has, however, its own particular adminiſtration and juriſdiction; the ordinances, by which its members are governed, are regulated by itſelf: It has the right of cenſure both with regard to the books printed by itſelf, or thoſe brought from abroad. All articles which it may want are allowed to paſs the frontiers without examination or tax. Its correſpondence is poſt free, and its paper is not ſubject to duty. The houſes of the profeſſors are free from taxes and all charges. The profeſſors have the rank of the ſeventh claſs, and the ſtudents that of the twelfth, or that of ſubaltern officers, receive commiſſion as ſuch, and wear ſwords. The profeſſors after twenty-five years duty, or in caſe of incurable ſickneſs, receive their penſions for life, and may even receive them while reſident in other countries. On the death of a profeſſor, his widow and children continue to receive his penſion, until the widow marries again, or the children attain the age of twenty-one years. The Emperor has granted a yearly revenue to the Univerſity of 130,000 rubles.

Forms its own ordinances and regulations. Various privileges.

Proviſion for profeſſors, &c.

Botanic Garden, &c. at Copenhagen.

A ſum of 4,500 rix dollars, which the government had granted in 1803, for the Botanic Garden at Copenhagen, has been employed, partly in paying the debts of the eſtabliſhment, and partly in conſtructing a new hot-houſe. This garden, which poſſeſſes 5,500 plants of different kinds, is open one day in each week for the curious, and every day for botanic ſtudents. The directors in their laſt report, having made ſome propoſals for the improvement and eſtabliſhment of the garden, the government has granted them an additional ſum of 4000 rix dollars, and an annual ſum of 200 for repairs; and have beſides ſettled, that the appointments of perſons employed in the garden, ſhall be increaſed to 720 rix dollars, to commence this year.

has 5500 plants for inſpection,

additional grants made to it.

M. Gieſecke, a Pruſſian mineralogiſt, who has been a conſiderable time at Copenhagen, is about to be employed by government on a voyage to Greenland, where he is to paſs ſome years in examining that country, with regard to its mineralogy and geology. Hitherto the Moravian religious miſſionaries have alone been able to reſolve to live ſome years

Propoſed mineralogical expedition to Greenland.

years in that country for the converſion of the natives: It will be no little honour to the ſciences, if M. Gieſecke ſhall bring himſelf to make a like ſacrifice for their advancement.

Charts printed by moveable types.

The Royal Academy of the Fine Arts, and the Mechanical Arts of Berlin, have received among their members M. F. H. Wagener, who has diſcovered a new method of printing geographical charts by a ſpecies of moveable types, which is found to anſwer better than engraving, and will undoubtedly be much cheaper.

Prizes given by the French war miniſter for a work on ſubterranean fortification,

Marſhal Berthier, war miniſter of France, at the requeſt of general Mareſcot, has again eſtabliſhed the prizes which was given for the beſt works on fortification.

Two prizes have been granted to the beſt *treatiſes on ſubterraneous works*. The firſt was adjudged by the committee of fortification to Major Mouze, the ſecond to Captain Gillot; the committee has adjudged a third treatiſe deſerving of honourable mention, which has for its inſcription *artem experientia fecit;* the author of it is not known.

and one on a plan for a fortified barrack.

Another prize on the ſubject of a *project for a fortified barrack*, has been given to Captain Laurent. The committee have judged the project of Captain Bioſchevalier, and that of Lieutenant Colonel Geſbert, to deſerve honourable mention. The committee has rejected, for not correſponding with the propoſed ſubject, a project of Captain Mallet, for *a barrack intrenchment*; but have thought it worthy of particular mention, as a work which gives a very advantageous idea of the talents of this officer.

Many of the works which neither received prizes, or particular mention, exhibit ingenious contrivances, and intereſting obſervations. In general theſe two conteſts have fully proved the goodneſs of the Inſtitution, of which the object is to exite emulation in all the corps of the army, to propagate knowledge among them, and to extend the perfection of all the branches of the military art.

Catalogue of Leipſic fair, contained 3647 publications.

THE catalogue of the Leipſic fair, has this year contained two ſheets more than uſual. The muſical publications have been

been added to it. It contains 3,647 articles, furniſhed by 380 bookſellers. The number of romances is 271, of theatrical pieces 81, and muſic 95, forms 447 articles.

Publication of Arabic antiquities of Spain.

The academy of painting and ſculpture at Madrid, are about to publiſh a compleat collection of the Arabic antiquities of the kingdoms of Grenada and Cordova. In this work will be found not only views and plans of the monuments, and other remarkable matters of theſe countries, but alſo an explanation of all the inſcriptions, cyphers and hieroglyphics.

Dictionary of the Bunda language, ſerviceable to thoſe who have buſineſs in Africa.

THERE is ſoon to be publiſhed at Liſbon, a Dictionary of the Angola or Bunda language, with the explanation of all the words in Portugueſe. There has never before been a dictionary of this language. This will be publiſhed for the uſe of thoſe Portugueſe who have buſineſs to tranſact with the eſtabliſhments which their country poſſeſſes on the coaſt of Africa. No language is ſpoken there to ſo great an extent as this.

Mauſoleum of Chriſtina, by M. Canova,

THE celebrated ſculptor M. Canova, is engaged in erecting at Vienna, the ſplendid Mauſoleum of the Arch-dutcheſs Chriſtina, an immenſe compoſition of eight marble figures, larger than life; the models and execution of which have been long admired at Rome, where they were formed. M. Canova before his departure from Rome exhibited a coloſſal group, repreſenting Theſeus combating with a Centaur. This group is to be executed in marble for the city of Milan. The artiſts and connoiſſeurs of Rome ſeem to eſteem this work ſuperior to every other which has been executed by this ingenious and indefatigable artiſt.

and his Theſeus and Centaur.

Piracy of books puniſhed at Augſburg.

THE Magiſtracy of Augſburg have had the honour of being the firſt government of ſouth Germany, which have taken deciſive meaſures againſt the ſhameful traffic of book-piracy. It has confiſcated the entire edition, conſiſting of 500 impreſſions, of the work of Goener, on the political rights

rights of Germany, which was pirated by Krauzfelder, a dealer in ſuch tranſactions, and has beſides compelled Krauzfelder to pay to the legitimate editor, the price of the copies which he had ſold.

Ruſſian marine Inſtitution.

THE Ruſſian government have formed at Peterſburg, an Inſtitution, whoſe deſign is the perfecting of all that belongs to naval armaments, and which is to be called the *Marine Muſeum*. This inſtitution is not merely to be a ſchool: all the ſciences neceſſary to a naval officer will be there taught and the *Muſeum* will beſides publiſh a journal which will treat of every thing relative to the marine. It will have alſo a cabinet of natural hiſtory, which will be open to all the pupils. This eſtabliſhment will be under the direction of the miniſter of the marine, and its members are to wear the naval uniform.

Ruſſian eſtabliſhments for education. Numbers of maſters and pupils.

ACCORDING to the report of the miniſter of public inſtruction, there is at preſent in Ruſſia 494 inſtitutions for education, directed by 1475 maſters, and attended by 33484 ſcholars. The expence of theſe eſtabliſhments coſts government annually almoſt two millions of Rubles. Among theſe are not reckoned thoſe for the corps of cadets, or for pages, the Academy of Arts, the Schools of Commerce, nor the Inſtitution for Female Education. Thoſe who know the ſtate in which Ruſſian education was at the acceſſion of Alexander, may judge by this detail what he has done towards enlightning his vaſt empire.

The Ruſſian catholics earneſtly concur in ſeconding his views. At an eccleſiaſtical aſſembly, convoked by the biſhop of Luſk and Shetomir, various meaſures have been taken in favour of the eſtabliſhments for education.

Theatre in the Crimea.

IN the town of Odeſſa in the Crimea, a theatre is building with much activity, according to the plans of M. Thomas de Thomon architect to the Emperor, and a profeſſor of the Peterſburg academy.

This

(This ſhews that the arts are even extending to this hitherto neglected part of the world, which certainly from its fine climate, and many other advantages, merits every attention of its enlightened and humane maſter.)

THE third volume of the *Geographical Dictionary of the Ruſſian Empire* has been publiſhed by the bookſellers, Gavy, Popow, and Luby. Geographical Dictionary of Ruſſia.

AN important work is ſoon expected to appear at Peterſburg, by the ſcientific M. Delaunay, counſellor of ſtate, relative to Siberia, and the bordering countries.

M. KOTZEBUE in his laſt tour to Naples relates ſome particulars which he ſaw in viſiting the Muſeum of Portici, which will be intereſting to the admirers of ancient literature. Ancient manuſcripts diſcovered at Portici are

" Eleven young men are at preſent employed in unrolling the manuſcrips, and two copy them. An Engliſhman called Haiter, is at the head of the eſtabliſhment. He relates that his aſſiſtants are much more expert and expeditious than they were formerly. He has great hope that he ſhall have the 600 manuſcripts, (which yet remain) decyphered, and has little doubt that he ſhall diſcover among them an *Ennius* and a *Menander*, as he flatters himſelf he has already a *Polybius* in hands.

On the day of the viſit, a Greek author, hitherto unknown named Kolotos, was diſcovered; his work is on philoſophy. As the names of the author are always inſerted at the end of each manuſcript, they can never be known until it is entirely unrolled. Seven latin authors have paſſed through the hands of Mr. Haiter, but all ſo much damaged that it was impoſſible to unroll them, which he the more laments as one of them appeared to be a *Livy*, at leaſt it was an hiſtorical work written in his ſtyle; all that he can diſcover of it, is, that it begins with an harangue, in which mention is made of the family of Acilius. They have to this time diſcovered five authors; *Philodemus*, moſt of whoſe writings have been found, and among others, a treatiſe on the *vices which are nearly allied to virtues*. *Epicurus*, *Phædrus*, *Demetrius Phalereus* and the works of Epicurus, Phædrus,

Demetrius, and a Kolotos.

and his *Kolotos* above-mentioned. Mr. Haiter regrets that he has hitherto only met with works on philoſophy, although among theſe many hiſtorical ideas, hitherto unknown, occur here and there: as happened in a diſſertation on anger, in which is cited the example of Cadmus puniſhed by Bacchus for having given himſelf up to this paſſion, a circumſtance hitherto unknown.

IT has for ſome time been an object of deliberation with me, to aſcertain by what means I might moſt effectually remedy an inconvenience which has ariſen from the diſtinguiſhed patronage this Journal has been honoured with: The great extent and value of original communications cannot but be duly eſtimated by the public; though at the ſame time it has neceſſarily followed, that various articles of news and other ſubjects in the foreign Journals, have in many inſtances been poſtponed, and in ſome rejected. To retain all the peculiar advantages of this work, and to afford ample ſpace for occaſional and foreign articles of value, the obvious means have appeared, that according to the practice of ſeveral other reſpectable works, each volume ſhould be concluded by a Supplementary Number; containing ſix ſheets, or 96 pages of printed matter, and two plates. And, as many of the former plates, like thoſe in the preſent number, have contained mathematical figures or outline delineations, capable of being advantageouſly condenſed, it is purpoſed in all the future numbers to give two very full plates, and ſixteen extra pages of matter, inſtead of the four plates hitherto given. By this arrangement every volume will in future contain 30 ſheets or 480 pages of matter, and 10 full plates; inſtead of 20 ſheets or 320 pages, with 16 plates leſs fully occupied. This addition of new matter to the amount of full one half more, will admit the inſertion of many intereſting articles which want of room muſt alſo have excluded.

*** *The plate of Rye-Harbour could not be finiſhed in time on account of the ſudden illneſs of the Engraver. It will be given gratis in the Supplement, which will be publiſhed Jan.* 1. *next, at the ſame time as No.* 50.

Mr. Gough's Propositions respecting a Division of the Arch of a Circle.

Fig. 2.

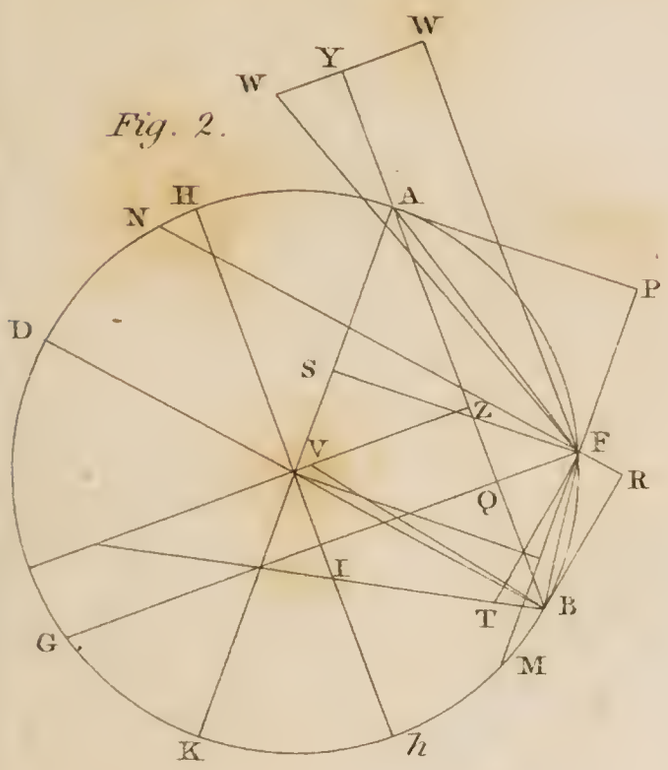


Fig. 3.

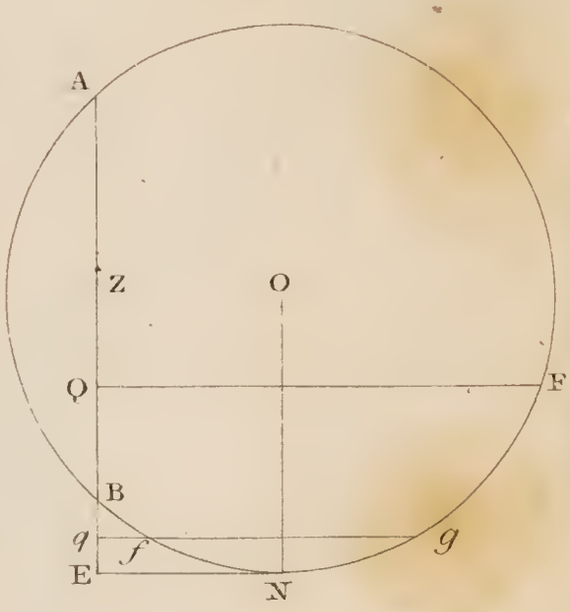


Fig. 1.

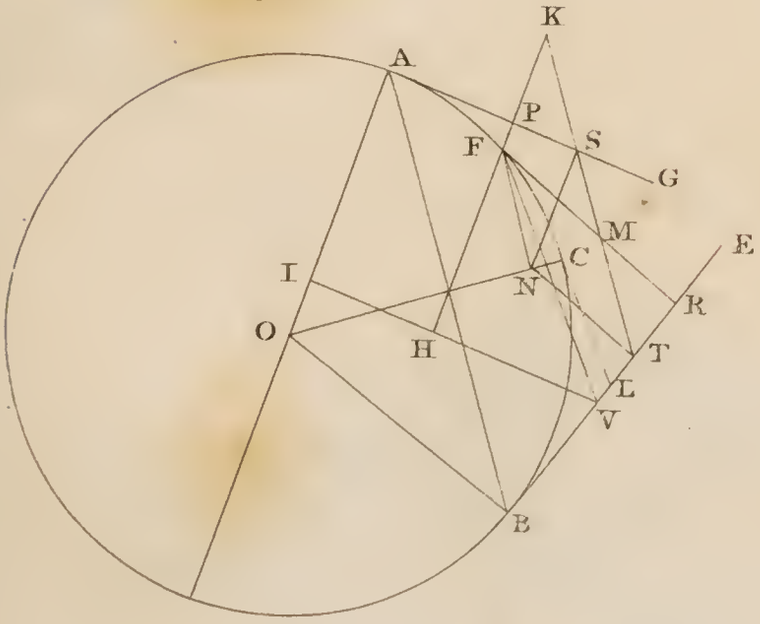
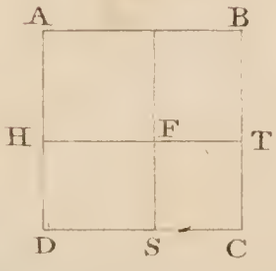


Fig. 4.



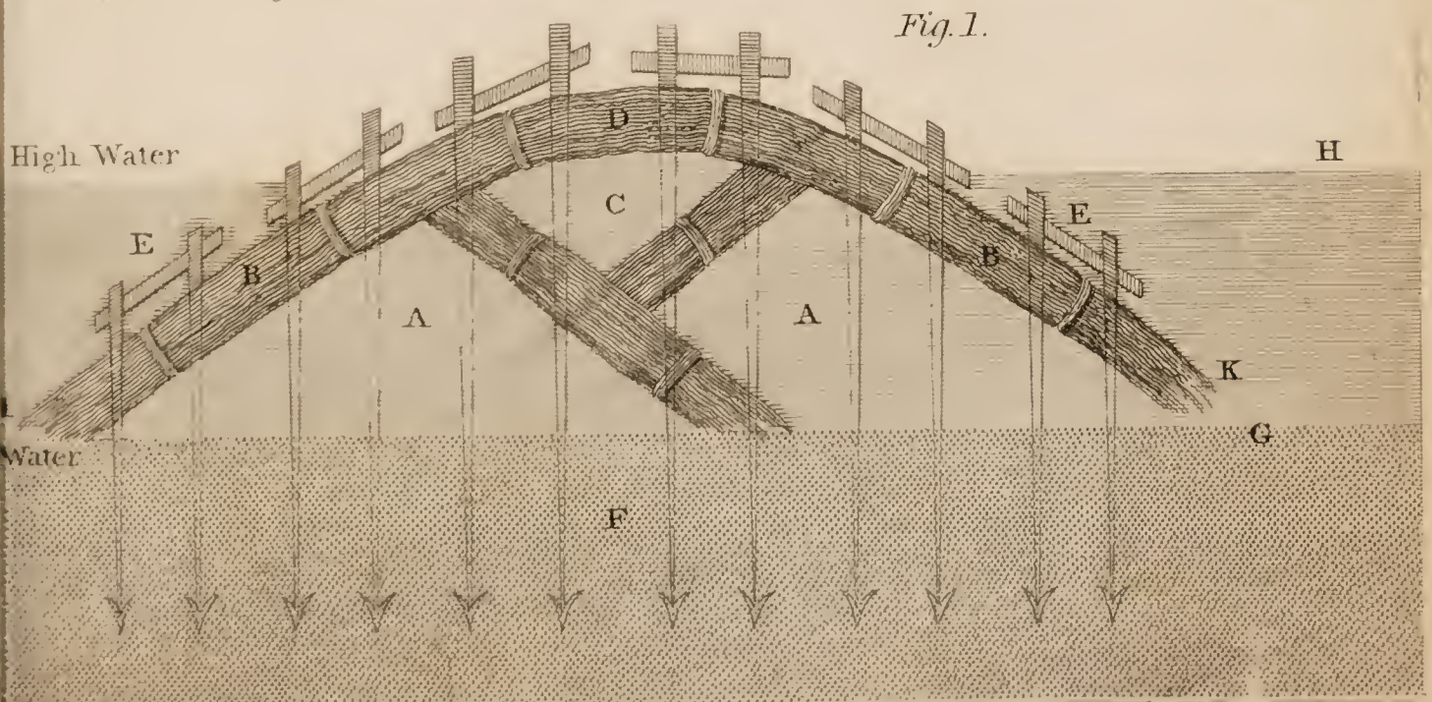
Mathew Sc. Russell Col.

Fig. 2.

Rev.d D. Pape's improvements of Rye Harbour.

Cross Section of Mr. Pape's Dam.

Fig. 1.



Mut. sc. So. Rupert Coe

Dr. Herschell's investigation of the power of telescopes to distinguish the figures of very minute objects.

Fig. 1.

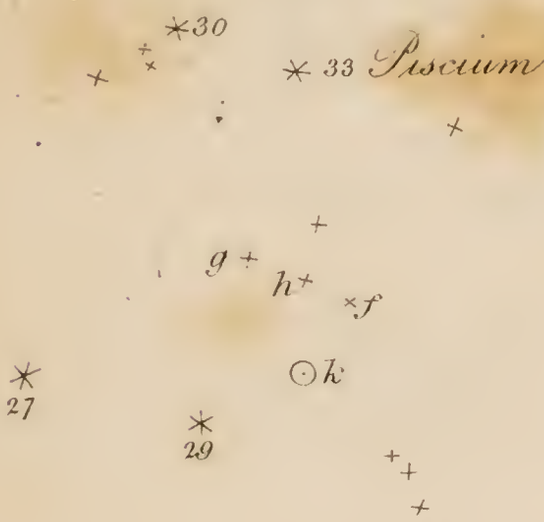


Fig. 2.

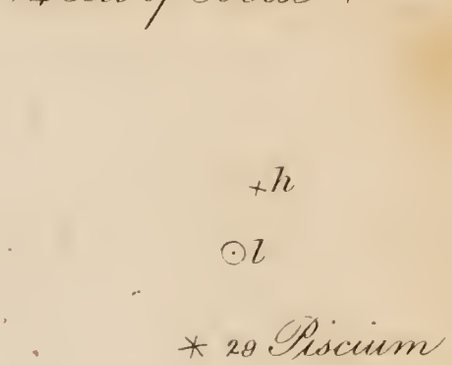


Fig. 3.

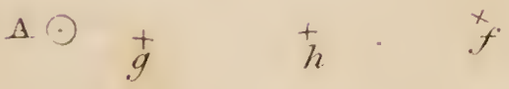
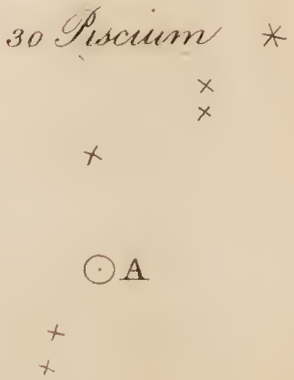


Fig. 4.



A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

SUPPLEMENT TO VOL. XII.

ARTICLE I.

A Deſcription of an Air Pump upon a new Conſtruction. By ELIZUR WRIGHT. *Communicated by* BENJ. SILLIMAN, *Eſq. Profeſſor of Natural Philoſophy, &c. in Yale College, Newhaven, America.*

The general imperfections of the air pump explained.

UPON reading the improvements made in the air pump by Smeaton, Haas, Prince, Ruſſel, and Cuthbertſon, it occurred to me that the end which they aimed at might in ſome meaſure be attained upon a principle that is different from either of thoſe by which their pumps have been conſtructed It is well known that in a common air pump the valve at the bottom of the barrel depends upon the air in the receiver to open it. When the air in the receiver is rarified to a certain degree, its ſpring becomes too weak to overcome even the ſmall reſiſtance which will ariſe from the weight of the valve, its coheſion to the plate occaſioned by the oil, and its being ſtretched tight over the hole. Here the progreſs of exhauſtion will ſtop. And this would hold true, could it be poſſible to produce a perfect vacuum in the barrel. But as the ſame obſtructions belong to the piſton valve, together with the additional one ariſing from the preſſure of the external air upon it, and becauſe the piſton can-

not be ſo accurately fitted to the lower valve as, when put down upon it, to leave no vacuity between them; a portion of air will neceſſarily be retained in the barrel, which by its preſſure ſtill further prevents the opening of the lower valve, and cauſes the operator to come to the limit of rarefaction much ſooner than he would upon the ſuppoſition that a perfect vacuum were made in the barrel. Several very ingenious contrivances have been invented to remove this imperfection, among which thoſe of Cuthbertſon and Prince are among the lateſt, and cannot fail of giving the reader a very high idea of their ſagacity and talents for invention. The method uſed by the Rev. Mr. Prince, of removing the lower valve by opening the bottom of the barrel into a ciſtern which has a communication with the receiver, firſt gave the hint that it might be poſſible in ſome ſimilar way to diſpenſe with both the valves, and by this means carry the air pump to a greater degree of perfection. In purſuing this ſubject I found that all this might be effected, and in a way that admitted of much ſimplicity of conſtruction.

Improvements of Cuthbertſon and Prince.

The air-pump of Prince improved.

Deſcription of a new air pump.

The principle upon which this pump operates may be ſeen in the following deſcription of it. F, (*Plate* XIV. *Fig.* 1.) is the pump plate. O C is the barrel lying in a horizontal poſition underneath the pump plate, and nearly in contact with it. A and B are two ducts leading from the pump plate into the barrel. The piſton P is without a valve, being ſolid and accurately fitted to the barrel. The piſton rod M is cylindrical and moves air tight in the leathern collar O. There is another piſton, N, made like the former, but ſhorter, and acted upon by the ſpring S, which is thence termed the ſpring piſton. The ends of theſe piſtons are very carefully fitted to each other, ſo that when they are brought into contact they will form one uniform cylinder without any vacuity betwixt them. H is the winch, with a pinion and rack by which it is worked. The pump is ſupported by a pedeſtal upon which it is firmly fixed.

A ſolid piſton works in a barrel. Its rod paſſes through a collar of leather. Two holes in the barrel ſerve, one to admit air from the receiver and the other to diſcharge it.

The manner in which it operates is this: Suppoſe the receiver placed over the duct A, leaving the duct B open to the external air, alſo the ſpring piſton in the ſituation N, excluding the external air from the barrel, as repreſented in the figure and the piſton P in contact with it. The piſton P, by moving towards the duct A, forms a vacuum in the barrel. When it paſſes by the duct A, it opens a communication between the

receiver

receiver and barrel, and the air by its elaſtic force ruſhes into the barrel and fills it. The piſton now returns towards the duct B, and drives before it the air contained in the barrel, together with the ſpring piſton N, until they are ſtopped by the ſhoulder D at the inſtant in which the ends of the two piſtons come againſt the middle of the duct B. By forcing the air out at the duct B, the piſtons come into contact, and form one uniform cylinder, that prevents any communication of the barrel with the external air. The piſton P is now drawn back toward the duct A, and the ſpring piſton N, by the action of its ſpring, follows in cloſe contact with it, until it is ſtopped by its ſhoulder C meeting with the end of the barrel, after having paſſed the duct B, and having continued to intercept the communication between the barrel and the external air. This is the ſituation with which the deſcription began; and, repeating the operation, when the piſton P is drawn back beyond the duct A, the air from the receiver ruſhes into the barrel; and when it moves forward to the duct B this air is expelled.

The latter is cloſed by a ſpring piſton.

Having exhibited a general deſcription of this pump, with the manner of its working, a more particular illuſtration of ſome of its parts will be given.

When the pump is intended to exhauſt, the receiver muſt be placed over the duct A, leaving the duct B open to the external air; but when it is deſigned to condenſe, nothing more is neceſſary than to ſhift the ſituation of the receiver on the plate, placing it over the duct B, and leaving the duct A open to the external air.

This pump exhauſts or condenſes at pleaſure.

The duct B is continued around the ſpring piſton by means of a circular channel cut into the inſide of the barrel, in order that the air might eſcape from all ſides when the piſtons come into contact.

It may be obſerved that all the back ſpace in the barrel between the collar O and the piſton P makes a part of the capacity of the receiver; or, to ſpeak more accurately, the ſpace O A between the collar O and the duct A: the ſpace A P between the duct and piſton, while it moves from A toward B, being only a temporary dilation of the capacity, and the ſpace A P while it moves from A towards O a temporary contraction of it.

Obſervations.

For the purpoſe of preventing a fluctuation of the air in the receiver, which would be cauſed by this expanſion and contraction,

Advantage of conſtructing it with two barrels.

traction, and might be detrimental in ſome experiments, the diameter of the duct A is made very ſmall, and another barrel, having ſimilar piſtons and ducts, is added, with its rack placed above the pinion wheel, while the other is placed below it. The advantage of a pump of this kind being conſtructed with two barrels ariſes from the contrary motions of their piſtons; for while one augments the capacity of the receiver by moving forward, the other equally diminiſhes it by moving backward. An equilibrium is thus maintained that prevents any oſcillatory motion in the mercury of the gage, which might ariſe from the operation of a ſingle barrel.

The reſiſtance from the ſpring piſton.

The additional reſiſtance to be overcome in working this pump, above what is to be met with in other pumps, happens only at the ſmall interval while the ſpring piſton is paſſing from its natural ſituation to the duct B. This need not be more than about four times greater than that which is requiſite to overcome the friction of the piſton P, and will be eaſily provided or by increaſing the proportion between the diameter of the pinion wheel and the ſweep of the handle.

ELIZUR WRIGHT, *C. A. S.*

Canaan (Connecticut in America,)
March 12, 1805.

II.

Concerning the State in which the true Sap of Trees is depoſited during Winter. By THOMAS ANDREW KNIGHT, *Eſq.**

(Concluded from Page 240)

Bulbous and tuberous roots contain the matter that forms leaves,

WE have much more deciſive evidence that bulbous and tuberous rooted plants contain the matter within themſelves which ſubſequently compoſes their leaves; for we ſee them vegetate even in dry rooms, on the approach of ſpring; and many bulbous rooted plants produce their leaves and flowers with nearly the ſame vigour by the application of water only, as they do when growing in the beſt mould. But the water in this caſe, provided that it be perfectly pure, probably affords little or no food to the plant, and acts only by diſſolving the

* Phil. Tranſ. of 1805, p. 97.

matter

matter prepared and depoſited in the preceding year; and hence the root becomes exhauſted and ſpoiled: and Haſſenfratz found that the leaves and flowers and roots of ſuch plants afforded no more carbon than he had proved to exiſt in bulbous roots of the ſame weight, whoſe leaves and flowers had never expanded.

As the leaves and flowers of the hyacinth, in the preceding caſe, derived their matter from the bulb, it appears extremely probable that the bloſſoms of trees receive their nutriment from the alburnum, particularly as the bloſſoms of many ſpecies precede their leaves: and, as the roots of plants become weakened and apparently exhauſted, when they have afforded nutriment to a crop of ſeed, we may ſuſpect that a tree, which has borne much fruit in one ſeaſon, becomes in a ſimilar way exhauſted, and incapable of affording proper nutriment to a crop in the ſucceeding year. And I am much inclined to believe that were the wood of a tree in this ſtate accurately weighed, it would be found ſpecifically lighter than that of a ſimilar tree, which had not afforded nutriment to fruit or bloſſoms, in the preceding year, or years.

—and it is highly probable that trees contain the nutriment of their fruits, &c.

If it be admitted that the ſubſtance which enters into the compoſition of the firſt leaves in the ſpring is derived from matter which has undergone ſome previous preparation within the plant, (and I am at a loſs to conceive on what grounds this can be denied, in bulbous and tuberous rooted plants at leaſt,) it muſt alſo be admitted that the leaves which are generated in the ſummer derive their ſubſtance from a ſimilar ſource; and this cannot be conceded without a direct admiſſion of the exiſtence of vegetable circulation, which is denied by ſo many eminent naturaliſts. I have not, however, found in their writings a ſingle fact to diſprove its exiſtence, nor any great weight in their arguments, except thoſe drawn from two important errors in the admirable works of Hales and Du Hamel, which I have noticed in a former memoir. I ſhall therefore proceed to point out the channels, through which I conceive the circulating fluids to paſs.

The preparation of this nutriment in the tree implies that the juices circulate.

When a ſeed is depoſited in the ground, or otherwiſe expoſed to a proper degree of heat and moiſture, and expoſure to air, water is abſorbed by the cotyledons and the young radicle or root is emitted. At this period, and in every ſubſequent ſtage of the growth of the root, it increaſes in length by the addition of new parts to its apex, or point, and not by any general diſtenſion

Explanation of the manner in which the juices of plants circulate, their habitudes, changes, &c.

Explanation of the manner in which the juices of plants circulate, their habitudes, changes, &c.

tenſion of its veſſels and fibres; and the experiments of Bonnet and Du Hamel leave little grounds of doubt, but that the new matter which is added to the point of the root deſcends from the cotyledons. The firſt motion therefore of the fluids in plants is downwards, towards the point of the root; and the veſſels which appear to carry them, are of the ſame kind with thoſe which are ſubſequently found in the bark, where I have, on a former occaſion, endeavoured to prove that they execute the ſame office.

In the laſt ſpring I examined almoſt every day the progreſſive changes which take place in the radicle emitted by the horſe cheſnut: I found it, at its firſt exiſtence, and until it was ſome weeks old, to be incapable of abſorbing coloured infuſions, when its point was taken off, and I was totally unable to diſcover any alburnous tubes, through which the ſap abſorbed from the ground, in the ſubſequent growth of the tree, aſcends: but when the roots were conſiderably elongated, alburnous tubes formed; and as ſoon as they had acquired ſome degree of firmneſs in their conſiſtence, they appeared to enter on their office of carrying up the aqueous ſap, and the leaves of the plumula then, and not ſooner, expanded.

The leaf contains at leaſt three kinds of tubes: the firſt is what, in a former Paper, I have called the central veſſel, through which the aqueous ſap appears to be carried, and through which coloured infuſions readily paſs, from the alburnous tubes into the leaf-ſtalk. Theſe veſſels are always accompanied by ſpiral tubes, which do not appear to carry any liquid: but there is another veſſel which appears to take its origin from the leaf, and which deſcends down the internal bark, and contains the true or prepared ſap. When the leaf has attained its proper growth, it ſeems to perform preciſely the office of the cotyledon; but being expoſed to the air, and without the ſame means to acquire, or the ſubſtance to retain moiſture, it is fed by the alburnous tubes and central veſſels. The true ſap now appears to be diſcharged from the leaf, as it was previouſly from the cotyledon, into the veſſels of the bark, and to be employed in the formation of new alburnous tubes between the baſe of the leaf and the root. From theſe alburnous tubes ſpring other central veſſels and ſpiral tubes, which enter into and poſſibly give exiſtence to, other leaves; and thus by a repetition of the ſame

proceſs

proceſs the young tree or annual ſhoot continues to acquire new parts, which apparently are formed from the aſcending aqueous ſap.

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But it has been proved by Du Hamel that a fluid, ſimilar to that which is found in the true ſap veſſels of the bark, exiſts alſo in the alburnum, and this fluid is extremely obvious in the fig, and other trees, whoſe true ſap is white, or coloured. The veſſels, which contain this fluid in the alburnum, are in contact with thoſe which carry up the aqueous ſap; and it does not appear probable that, in a body ſo porous as wood, fluids ſo near each other ſhould remain wholly unmixed. I muſt therefore conclude that when the true ſap has been delivered from the cotyledon or leaf into the returning, or true ſap veſſels of the bark, one portion of it ſecretes through the external cellular, or more probably glandular ſubſtance of the bark, and generates a new epidermis, where that is to be formed; and that the other portion of it ſecretes through the internal glandular ſubſtance of the bark, where one part of it produces the new layer of wood, and the remainder enters the pores of the wood already formed, and ſubſequently mingles with the aſcending aqueous ſap; which thus becomes capable of affording the matter neceſſary to form new buds and leaves.

It has been proved in the preceding experiments on the aſcending ſap of the ſycamore and birch, that that fluid does not approach the buds and unfolding leaves in the ſpring, in the ſtate in which it is abſorbed from the earth: and therefore we may conclude that the fluid, which enters into, and circulates through the leaves of plants, as the blood through the lungs of animals, conſiſts of a mixture of the true ſap or blood of the plant with matter more recently abſorbed, and leſs perfectly aſſimilated.

It appears probable that the true ſap undergoes a conſiderable change on its mixture with the aſcending aqueous ſap; for this fluid in the ſycamore has been proved to become more ſenſibly ſweet in its progreſs from the roots in the ſpring, and the liquid which flows from the wounded bark of the ſame tree is alſo ſweet; but I have never been able to detect the ſlighteſt degree of ſweetneſs in decoctions of the ſycamore wood in winter. I am therefore inclined to believe that the ſaccharine matter exiſting in the aſcending ſap is not immediately, or wholly, derived from the fluid which had circulated through

Explanation of the manner in which the juices of plants circulate, their habitudes, changes, &c.

through the leaf in the preceding year; but that it is generated by a proceſs ſimilar to that of the germination of ſeeds, and that the ſame proceſs is always going forward during the ſpring and ſummer, as long as the tree continues to generate new organs. But towards the concluſion of the ſummer I conceive that the true ſap ſimply accumulates in the alburnum, and thus adds to the ſpecific gravity of winter-felled wood, and increaſes the quantity of its extractive matter.

I have ſome reaſons to believe that the true ſap deſcends through the alburnum as well as through the bark, and I have been informed that if the bark be taken from the trunks of trees in the ſpring, and ſuch trees be ſuffered to grow till the following winter, the alburnum acquires a great degree of hardneſs and durability. If ſubſequent experiments prove that the true ſap deſcends through the alburnum, it will be eaſy to point out the cauſe why trees continue to vegetate after all communication between the leaves and roots, through the bark, has been intercepted: and why ſome portion of alburnous matter is in all trees* generated below inciſions through the bark.

It was my intention this year to have troubled you with ſome obſervations on the reproduction of the buds and roots of trees; but as the ſubject of the Paper, which I have now the honour to addreſs to you, appeared to be of more importance, I have deferred thoſe obſervations to a future opportunity; and I ſhall at preſent only obſerve, that I conceive myſelf to be in poſſeſſion of facts to prove that both buds and roots originate from the alburnous ſubſtance of plants, and not, as is, I believe, generally ſuppoſed, from the bark.

I am, &c.

T. ANDREW KNIGHT.

Elton, Dec. 4, 1804.

* I have in a former paper ſtated that the perpendicular ſhoots of the vine form an exception. I ſpoke on the authority of numerous experiments; but they had been made late in the ſummer; and on repeating the ſame experiments at an earlier period, I found the reſult in conformity with my experiments on other trees.

Singular

III.

Singular Method of forming Walls and Roofs of Rural Building, in Indoſtan, communicated by M. LEGOUX DE FLAIX, *Officer of Engineers* *.

THE method which the Indians have uſed for many years, of forming their rural buildings, unites ſolidity, convenience, and wholeſomeneſs to economy, and facility of execution. Advantages of this method of building.

Houſes conſtructed in this manner have alſo the advantage of being abſolutely ſafe from conflagration, and of reſiſting even the moſt violent inundations. Reſiſts fire and inundations.

In a country where ſtone is ſcarce, the rich build their houſes with bricks, which in many reſpects are preferable to ſtone; but poor people, ſuch as thoſe employed in agriculture, cannot go to that expence, even in India, where labour and materials are ſo cheap.

The habitations of villagers in moſt parts of the globe are built with earth walls, in India they are likewiſe covered with terraces of earth, and it is evident, that buildings formed with both walls and roofs of earth muſt neceſſarily be free from danger of fire.

To prove that buildings of this conſtruction are equally ſafe from inundations, it is ſufficient to ſtate, that on the banks of the Ganges and Indus, (rivers of vaſt magnitude both in their extent and their courſe, and whoſe great bodies of water cauſe the moſt deſtructive effects in their floods,) theſe houſes ſtand uninjured, though ſometimes iſolated in the midſt of immenſe inundations for fifteen or twenty days. It is extremely probable, that houſes built of ſtone or brick would not ſtand this great force of water equally well.

To form houſes in this manner, the foundations of the outſide and partition walls are dug up, which are ſometimes from five to ſeven feet deep, and always proportioned to the heighth intended to be given to the walls. The excavated earth is expoſed till it becomes perfectly dry; if it is of a fat or argillaceous nature, it is carried to a place, prepared for the purpoſe, where it may be pounded into a duſt, and properly prepared for uſe; when in this ſtate, it is mixed with a The earth dug from the foundations is pounded fine, and mixed with coarſe ſand or fine gravel,

* Sonninus's Journal, II. 394.

third or an half of coarſe ſand, or ſmall gravel paſſed through a ſieve to clear it from pebbles. The fat earth is mixed with the ſand and gravel, and worked up well with it, ſo that the maſs may be of an uniform conſiſtence. It is then moiſtened with water five or ſix hours before it is wanted, and in the quantity neceſſary for a ſingle day's work alone.

then moiſtened with water.

The walls are raiſed all together,

The mixture thus prepared is carried to the place of building, when the foundations are perfectly dry, and the walls are then built equally in every part at the ſame time, on a perfect level, in courſes, and brought up perpendicularly: each courſe of earth is from eight to ten inches in depth, and the whole breadth of the wall, which is ſeldom leſs than two feet thick, and never exceeds four; which dimenſions are always regulated by the intended height of the building, and the force of the floods, if it is near the river. When the walls are three feet and a half, or four feet thick, only one courſe is raiſed in a day; but when they are from two feet and half to three feet thick, two courſes are raiſed, and if they are but two feet thick, three courſes are ſometimes raiſed in that ſpace of time. This depends on the quickneſs of the deſiccation of the walls, which ſpeedily takes place there, where the dryneſs of the air is extreme: this would not perhaps happen in our moiſt climates; if this method of building ſhould be tried here, it would probably be neceſſary to leave them longer to dry, in order to obtain the requiſite tenacity.

from two to four feet thick.

In one two or three courſes in a day according to the thickneſs.

Spaces left for beams, doors, and windows.

When the walls are built to the height for the roof, the proper openings are made for the beams and joiſts. It is almoſt needleſs to add, that the apertures neceſſary for the doors and windows are made while the walls are building.

The walls when dry are encloſed in open work caſes of bambou,

On the twelfth or fifteenth day, or when the walls are ſufficiently dry, or to the ſame degree to which tiles are dried, the walls are ſurrounded externally and internally with a ſort of open work caſe, made of ſpars of bamboù, or of ſome other hard and dry wood. In Indoſtan, where this method of building is general, the workman have bars of iron, which they hire out, that ſerve to ſuſtain the coffer work mentioned, and are placed at every three or four yards; the coffer work is raiſed at two or three feet diſtance from the ſurface of the walls, according to their thickneſs, and the ſpace between is filled up with firewood, turfs, and cakes made of cow and ſheep dung worked together and dried in the ſun.

at two or three feet diſtance, and the interval filled with fuel.

This

This pile of combuſtibles is arranged in ſeveral ſtages, of three, four, or five feet thick, ſeparated from each other by layers of earth or half dried turf of from eight to ten inches depth; the upper ſtages are firſt ſet on fire, ſo that the wall is baked through its whole extent from top to bottom. The charge of the combuſtibles for each of the ſtages is ſo managed, that the loweſt is the greateſt, and is diminiſhed for each as it is nearer the top of the wall; as the pile burns down the fire of the lower ſtages ſtill acts on the upper part of the walls, which permits the upper ſtages to be of leſs thickneſs. The fire bakes the walls to a thickneſs of from ſix to ten inches, as tiles are baked in a kiln. And thus walls are built in a ſingle piece, and of the greateſt ſolidity, which have the more ſtrength, as there are no junctures in them. Wherefore they ought to prevent the greateſt poſſible reſiſtance to the action of the atmoſphere, the attacks of floods, and the fall of rain, which deſcends in torrents in moſt countries of Indoſtan during the rainy ſeaſon.

Arranged in ſtages ſeparated by layers of earth.

The upper ſtages firſt kindled.

Walls thus baked to the thickneſs of from ſix to ten inches.

Experience has conſtantly proved, that the houſes built in this manner not only laſt much longer than thoſe built of bricks, but that they alſo reſiſt better the attacks of the periodical inundations, and thoſe of the annual rains to which they are expoſed in this climate.

Theſe houſes laſt longer than thoſe of brick, and reſiſt floods and rain better.

The method in which the terrace roofs (which are called in India *argamace*) are formed for theſe houſes, is the following:

Their roofs are made of clay in three layers.

Immediately after the baking and cooling of the walls, the aſhes and the bars which ſuſtained the coffer work are removed. The beams and joiſts are placed, and covered either with very thin boards, or elſe with ſmall green branches; and upon this ſupport the different layers of the terrace roof are placed. The firſt layer is ſimply clay, with an equal quantity of *ole*, a ſpecies of marl in powder, which is pounded in troughs, ſuch as are uſed for preparing mortar. This firſt layer is four or five inches thick, and it is then levelled, and is moiſtened from time to time, in order to beat it firm with ſmall bats. As ſoon as this is dry, the ſecond layer is laid on, which conſiſts of potters clay worked up in the ſame manner as if prepared for making pottery; this layer is only two or three inches thick at moſt. It is levelled according to the ſlope of the terrace, which is given it in placing the beams and

Firſt layer common clay and *ole*, a kind of marl, four or five inches thick.

Second layer potters clay, two or three inches thick.

and joiſts, and it is conſolidated by light blows of wooden trowels, until it is perfectly dry. When the clay forms cracks in drying they are cloſed by other clay prepared for filling up theſe chinks to the bottoms.

Third layer clay with one fourth brick duſt and one fourth fine ſand 6 or 8 inches thick.

The ſecond layer when perfectly dry, and free from cracks, is covered with a third layer; which is compoſed of pulverized clay mixed with a fourth of brick duſt, paſſed through a cloſe ſieve, and with a fourth of fine ſand. This mixture is worked up in a trough like mortar; it is uſed as ſoon as prepared, and is then ſpread out equally over the whole terrace ſix or eight inches thick; this layer is conſolidated in the ſame manner as the others, and this labour is continued till it is perfectly dry; and then the *argamace* is finiſhed. This terrace is ſtrong, and has ſuch tenacity that the moſt violent rains cannot penetrate it.

Houſes thus built coſt 6 franks for the cubic fathom.

A building of this ſpecies coſts in India but ſix francs (five ſhillings) for the cubic fathom and is entirely performed by maſons. In France it would coſt three times as much (and ſomething more in England) on account of the greater expence of labour and fuel.

May be made with many ſtories.

Houſes may be built in this manner of any height required, and of as many ſtories as are thought fit; I have ſeen ſome that had but one ground floor; but I have alſo ſeen others that were elevated two ſtories above the ground floor. One of this laſt ſort, ſituated on the banks of the Gemna in the province of Alabad, was built above 430 years, and the walls, and the whole of the building looked as freſh as if they were new.

An houſe of this ſort 430 years built ſeemed quite freſh.

IV.

Account of ſome new Improvements on Steam-Engines. By Mr. ARTHUR WOOLF.

(Concluded from page 296.)

Mr. Woolf's improvements in ſteam-engines.

"I have found out and invented a contrivance, by which the temperature of the ſteam veſſel or working cylinder of a ſteam-engine, or of the ſteam veſſels or cylinders where more than one are uſed, may be raiſed to any required temperature, without admitting ſteam from the boiler into any ſurrounding receptacle, whether known by the name of a ſteam caſe, or by any other denomination. That is to ſay, inſtead of admitting

ſteam

ſteam of a high temperature into ſuch receptacle or ſteam caſe, which is always attended with a riſk of exploſion proportioned to the elaſticity of the ſteam employed, I put into the ſaid ſurrounding receptacle, or caſe, oil or the fat of animals, or wax or other ſubſtances capable of being melted by a lower temperature than the heat intended to be employed, and of bearing that heat without being converted into vapour: or I put into the ſaid caſe or caſes mercury or mixtures of metals, as of tin, biſmuth, and lead, capable of being kept in a ſtate of fuſion in a lower temperature than that intended to be employed in working the ſteam-engine; and I ſo form the ſurrounding caſe or caſes as to make it or them admit the aforeſaid oil, or other ſubſtance employed, to come into contact not only with the ſides of the ſteam veſſel or veſſels, or working cylinder or cylinders, but alſo with the bottom and top of the ſame, ſo that the whole may be as much as poſſible maintained in one uniform temperature; and this temperature I keep up by a fire immediately under or round the caſe or caſes that contains the aforeſaid oil or other ſubſtance, or by connecting the ſaid caſe or caſes with a ſeparate veſſel or veſſels, kept at a proper temperature, filled with the oil or other ſubſtance made uſe of as aforeſaid. In ſome circumſtances, or whenever the ſame may be convenient or deſirable, I employ the fluid metals, or mixtures of metals, and oil or other of the ſubſtances before enumerated, at one and the ſame time in the ſame engine; that is to ſay, in the part of the caſe or veſſel expoſed to the greateſt action of the fire, I ſometimes have the aforeſaid metals or mixtures of metals, and in the parts leſs expoſed to the action of the fire, I put oil, or other ſubſtances capable of bearing the requiſite heat without being converted into vapour.

Mr. Woolf's improvements in ſteam-engines.

"By this arrangement, and method of applying the ſurrounding heat, I not only obviate the neceſſity of employing ſteam of a great expanſive force round the ſteam veſſel or veſſels, or the working cylinder or cylinders, as already mentioned, to maintain them at the temperature required; but I am enabled to obtain from ſteam of a comparatively low temperature, or even from water itſelf, admitted into the ſteam veſſel or veſſels, all the effects that can be obtained from ſteam of a high temperature, without any of

the

Mr. Woolf's improvements in ſteam-engines.

the riſk with which the production of the latter is accompanied, not only to the boiler and other parts of the machinery, but even to the lives of the workmen; for ſuch low ſteam, or even water, (but in every caſe ſteam is preferable,) being admitted into a ſteam veſſel or veſſels, or working cylinder or cylinders, kept at the requiſite higher temperature by the forementioned means, will there be expanded in any ratio required, and produce an effect in the working of the engine which cannot otherwiſe be obtained but at a greater expenſe of fuel, or with the riſk of an exploſion. By this means I can make uſe of ſteam expanded in any required ratio, or of any given temperature, without the neceſſity of ever having the ſteam of any greater elaſticity than equal to the preſſure of the common atmoſphere.

"Another improvement which I make uſe of in ſteam-engines conſiſts in a method of preventing, as much as poſſible, the paſſage of any of the ſteam from that ſide of the piſton which is acted upon by the ſaid ſteam to the other ſide which is open to the condenſer; and this I effect, in thoſe ſteam-engines known by the name of double engines, by employing upon or above the piſton mercury or fluid metal, or metals in an altitude equal to the preſſure of the ſteam. The efficacy of this arrangement will appear obvious, from attending to what muſt take place in working ſuch a piſton. When the piſton is aſcending, that is, when the ſteam is admitted below the piſton, the ſpace on its other ſide being open to the condenſer, the ſteam endeavouring to paſs up by the ſide of the piſton is met and effectually prevented by the column of metal equal or ſuperior to it in preſſure, and during the down ſtroke no ſteam can poſſibly paſs without firſt forcing all the metal through. In working what is called a ſingle engine a leſs conſiderable altitude of metal is required, becauſe the ſteam always acts on the upper ſide of the piſton. For ſingle engines, oil or wax, or fat of animals, or ſimilar ſubſtances, in ſufficient quantity, will anſwer the purpoſe, if another improvement, which conſtitutes part of my ſaid invention, be applied to the engine, namely to take care that in either the double or ſingle engine ſo to be worked, the outlet that conveys the ſteam to the condenſer ſhall be ſo poſited, and of ſuch a ſize, that the ſteam may paſs without forcing,

Mr. Woolf's improvements in ſteam-engines.

forcing before it or carrying with it any of the metal or other ſubſtance employed, that may have paſſed by the piſton; taking care at the ſame to provide another exit for the metal or other ſubſtance collected at the bottom of the ſteam veſſel or working cylinder to convey the ſame into a reſervoir kept at a proper heat, whence it is to be conveyed to the upper ſide of the piſton by a ſmall pump worked by the engine or by any other contrivance. In order that the fluid metal or metals uſed with the piſton may not be oxidated, I always keep ſome oil or other fluid ſubſtance on its ſurface, to prevent its coming in contact with the atmoſphere; and to prevent the neceſſity of employing a large quantity of fluid metal, I generally make my piſton of the depth of the column required, but of a diameter a little leſs than the ſteam veſſel or working cylinder, excepting where the packing or other fitting is neceſſary to be applied; ſo that, in fact, the column of fluid metal forms only a thin body round the piſton. In ſome caſes I make a hollow metallic piſton, and apply an altitude of fluid metal in the inſide of the working cylinder.

"It may be neceſſary, however, to ſtate, that in applying my improved method of keeping the ſteam veſſels of ſteam-engines at any required temperature to the engine known by the name of Savary's, in any of its improved forms, in which a ſeparate condenſer has been introduced, I ſometimes employ oil (or any other ſubſtance lighter than water, and capable of being kept fluid in the temperature employed, without being converted into vapour,) in the upper part of the tube or pipe attached to the ſteam veſſel; by which means ſteam of any temperature may be uſed without being expoſed to the riſk of partial condenſation by the admiſſion of any colder body into the ſteam veſſel; for the oil, or other ſubſtance employed for this purpoſe, ſoon acquires the requiſite temperature; and to prevent unneceſſary eſcape of heat, I conſtruct of, or line with, an imperfect conductor of heat, that part of the tube or pipe attached to the ſteam veſſel which may not be heated exteriorly. And further, (as is already the practice in ſome engines, and therefore not excluſively claimed by me,) I cauſe the water raiſed by the engine to paſs off through another aſcending tube than the one attached to the ſteam veſſel, but connected with it at ſome part lower than the oil or other ſubſtance employed in it is ever ſuffered to deſcend to in the working

working of the engine. The improvement which I have juſt mentioned, of introducing oil into the pipe attached to the ſteam veſſel of ſuch engines, may alſo be introduced without applying heat externally to the ſteam veſſel; but in this caſe part of the effect which would otherwiſe be gained is loſt."

V.

On the Magneſian Earth of Baudiſſero. *By* M. GIOBERT.

(Concluded from Page 284.)

The earth of Baudiſſero analyſed.

TO aſcertain the proportions of theſe conſtituent parts of the magneſian earth, we lixiviated a given weight of it, and precipitated the ſulphuric acid from one part of it by acetite of barytes, and the lime from the other part by oxalate of ammonia.

It contains, beſides magneſia, ſulphate of lime,

The weight of the oxalate of lime, and that of the ſulphate of barytes, obtained from it, ſhewed us that it contained 1,60 of the ſulphate of lime. The experiments before recited determine the proportion of the ſilex contained.

—with ſilex,

—and carbonic acid,

To prove that of the carbonic acid, we both calcined a given weight of the earth in crucibles, from which ſyphons paſſed into bottles containing lime water; in order that the carbonic acid gas furniſhed by the earth might be precipitated, and alſo diſſolved conſiderable quantities of it in acids by the action of heat, and received the gas produced in bottles filled in like manner with lime water; the firſt method produced conſtantly the moſt. The carbonate of lime formed in theſe different experiments apprized us that 100 parts of the earth contained from 8 to 12 of carbonic acid, and ſometimes a little leſs in the ſtony ſpecies.

—and water.

If this weight of the carbonic acid be deducted from the loſs of weight which this earth ſuffers by the calcination in the fire, which was mentioned before, we ſhall then have the quantity of water which the earth contains. In collecting the reſults of the different experiments, it appears that the earth of Baudiſſero is compoſed of

Magneſia

Magnesia - - -	68
Carbonic acid - -	12
Silex - - -	15,60
Sulphate of lime - -	1,60
Water - - -	3
	100,20

Proportions tabulated.

It is from these results that I denominate this earth native magnesia. It is doubtless found mixed with a little silex; but if the title of native alumen is given to the alumenous earth of Halle in Saxony, which contains 24 parts of the sulphate of lime; if the name of native magnesia is given to that of Moravia, announced by Mitchael, of which 100 parts contain 50 of carbonic acid; it appears to me that the earth which I describe has a much better title to the name which I have given it.

It may be called with propriety native magnesia.

The earth of Baudissero affords a subject for interesting observations in the investigation of its origin. Many facts lead me to believe that this earth and the Corneen stone or Cacholong, described and analysed by my colleague Bonvoisin, are both of the same nature. It appears to me that Cacholong at a certain point of its decomposition forms what Bonvoisin calls the hydrophane of Piedmont, and that in its complete decomposition it forms the magnesian earth of which I here give the analysis. Bonvoisin has declared himself of an opinion precisely contrary to this; for he has supposed that this earth, far from being the product of the decomposition of Cacholong, is the element of its formation. Our colleague Gioanetti is of the same opinion. In these two hypotheses, the change of one earth into another is manifest, that is to say, the change of silex and alumen into magnesia in my method of considering the matter; (for it is principally of these two earths that Cacholong and Hydrophane are composed, from the analysis of Bonvoisin;) and the change of magnesia into alumen and silex according to the hypothesis of Bonvoisin and Gioanetti. As this subject appears to me to be very interesting, I intend to make a comparative analysis of these stones at the different degrees of their decomposition or entering into the state of agate *(agatisation)* which shall be the subject of another memoir.

Supposed to be produced by the decomposition of the Corneen stone or Cacholong.

And therefore that either alumen and silex is changed into magnesia, —or the converse.

There remains yet for me to examine the economical uſes for which this earth may be employed.

The experiment which I related in the beginning of this memoir, of the decompoſition of the ſulphate of iron by this earth, which produced an excellent ſulphate of magneſia, indicates one of the methods in which it might be uſed to advantage.

Sulphate of magneſia may be manufactured from it to advantage by the proceſs with ſulphate of iron, deſcribed before, more pure than that of commerce in general.

Twenty-five pounds of ſulphate of iron coſt only three francs with us, while the price of the ſame weight of ſulphate of magneſia is eight francs, from this it follows that this proceſs may be followed to advantage. To this may be added that the ſulphate of magneſia of commerce, being impure, and mixed with much ſulphate of ſoda, cannot be compared to that which may be procured in this manner, which equals the beſt ſalt from * * canal; ſo that in this compariſon the more pure ſulphate of magneſia thus obtained, may be valued at ten francs at leaſt, and in reality is worth more.

This however is not the beſt method to purſue, when the operator has it in his power to follow the others, which I am going to recite.

The following experiments make known two proceſſes much more economical.

In the firſt experiment I took two pounds of the earth of Baudiſſero, reduced to a coarſe powder, with the ſame quantity of the ſulphuret of iron of Brozo reduced to powder in like manner, I mixed them together carefully, and treated one half in a crucible on the fire, and the other half in an iron capſule.

Or by pounding theſe and heating to redneſs in crucibles:

In both the mixture heated to redneſs emitted ſparks, eſpecially on being ſtirred. It ſeemed to become reduced to a very fine powder; a ſort of boiling took place, produced doubtleſsly by the diſengagement of carbonic acid, and here and there appeared flames of ſulphur, which burned without exhibiting any ſign of the production of a ſulphuret. The ſulphurous odour was not however very troubleſome, from whence it appeared that the magneſia abſorbed with readineſs the ſulphuric and ſulphurous acids in proportion as they were formed by combuſtion. The mixture became of a blackiſh grey, or more properly a black; but which appeared grey from the white particles which ſtill remained mixed with it.

After

After being left three hours to cool, it was moiſtened with water and put away till next day, a part of it was then lixiviated; the ſolution being made clear and treated with ammonia, gave an abundant and very white precipitate. This circumſtance indicating that much of the magneſia was combined with ſulphuric acid in the operation, all the remainder was lixiviated. The very clear lixivium, evaporated properly, produced at the firſt cryſtalization a pound of ſulphate of magneſia in beautiful cryſtals. The remaining liquor gave on ſucceſſive evaporations a pound and half more of the ſame ſalt in fine cryſtals, very dry and very white. The liquor produced cryſtals to the laſt drop, and the mother-water never became foul.

Leaving to digeſt ſome time, moiſtened with cold water, Lixiviating, and cryſtalizing, fine cryſtals in abundance are had. The mother-water, evaporated ſucceſſively gives cryſtals to the laſt drop.

The mixture which remained after lixiviation was roaſted a ſecond time, and again produced ſulphate of magneſia: It was then thrown away, although apparently it would have yielded more ſulphate of magneſia after another torrification.

The reſiduum roaſted again and re-lixiviated gives more cryſtals.

In another experiment, pure ſulphur was uſed inſtead of the pyrites; it was eaſy to foreſee that the reſult would be the ſame; it was however deſirable to prove it; and the reſult was perfectly ſatisfactory.

Pure ſulphur uſed in place of the ſulphate of iron anſwers equally well.

The uſe then which may be made of this earth, conſiſts in forming with it ſulphate of magneſia. The means by which this may be done are perhaps the moſt ſimple poſſible. It is ſufficient to reduce to powder the earth and the ſulphur, or the ſulphuret of iron, where it can be eaſily procured, as may be done at Baudiſſero. Theſe ſubſtances ſhould be mixed in almoſt equal parts; for it is uſeful to proceed with an exceſs of the earth, and the more ſo, as its coſt is almoſt nothing: The mixture ſhould be torrified in an oven or kiln, heated to the degree at which ſulphur inflames, and when there appear no more jets of ſulphurous flames, the kiln is to be left to cool. The matter being then drawn out ſhould be moiſtened with water in ciſterns, and left for ſome days, only taking care to ſtir it in that time.

Proceſs for ſulphate of magneſia in the large way with magneſian earth and pyrites, ſimilar to the foregoing.

The part of the ſulphur which in burning had only paſſed to the ſtate of ſulphurous acid, oxigenates gradually, or the ſalt, which at firſt was but a ſulphite changes to a ſulphate. The matter is then to be lixiviated, in the ſame manner that is uſed for nitrous earths, the liquor ſufficiently evaporated, and left to cryſtalize by cooling.

Another proceſs in the large way, where pyrites are burned. The kiln may be covered with a heap of magneſian earth.

Another method may be followed in places where ſulphurets are worked; or where, as at Brozo, there is a manufacture of ſulphate of iron. The kiln, where the pyrites are burned, may be covered with an heap of the magneſian earth; the ſulphuric acid, which is diſengaged will be abſorbed by the magneſia; and to the advantage of putting an end to the complaints of the owners of property near the manufacture, will be added that of *ſulphating* the magneſia, from which the ſalt may afterwards be procured by lixiviation. This laſt proceſs, if it were introduced into the manufactory of Brozo, would produce the ſulphate of magneſia of commerce at a very moderate price.

Which when *ſulphated* by the acid vapours, may be lixiviated.

The magneſian earth uſeful for porcelain and pottery,

As the magneſian earth of Baudiſſero forms an excellent porcelain with ſilex, it preſents beſides an intereſting ſubject for reſearch relative to the fabrication of pottery. With this earth and a quantity of the argillaceous earth of Caſtellamonte ſufficient to unite it into a paſte, I formed ſome crucibles and capſules. Theſe crucibles were expoſed for 48 hours in the furnace of the glaſs-houſe of Po. The earths did not ſeem to have formed a ſufficient union; nevertheleſs the hardneſs of the crucibles was ſuch that they could not be affected by the file. Doctor Gioanetti, who is now engaged in manufacture of ſtone-ware pottery, will hereafter throw light on this ſubject.

and mixed with argil forms crucibles extremely hard.

I end this part of my memoir with obſerving that the trials which I made of this earth as an abſorbent in veterinary medicine ſucceeded perfectly well.

Additions to the preceding Memoir, by the ſame.

The earth of Caſtellamonte is ſimilar to that of Baudiſſero.

Farther reſearches which I made on argillaceous earths have given me to underſtand that the earth of Baudiſſero is not the only one known that conſiſts for the moſt part of magneſia. The ſame kind is alſo found at Caſtellamonte, a large village near that of Baudiſſero.

M. Bertoline, doctor of medicine, one of the moſt eminent of my pupils, having repeated the detail of the experiments which we made at the general ſchool of chemiſtry, invited us to eſſay a particular earth of Caſtellamonte, his country, which he thought would furniſh the alumen which was ſought for unſucceſsfully in the earth of Baudiſſero; ſoon after, by the care of M. Onorato, ſurgeon of Caſtellamonte, who is the proprietor

prietor of the land where this earth is found, I received a large quantity of it, and we examined it comparatively with that of Baudiſſero.

The earth of Caſtellamonte, which was brought to us had nearly the ſame appearance as that of Baudiſſero; but when it is firſt dug up from the ground, it has on the other hand different external appearances, which ſeem to depend on the different degrees of decompoſition of the Corneen ſtone or Cacholong, which is found at Caſtellamonte as well as at Baudiſſero.

When firſt dug up appears different in colour.

The colour of this earth is a white inclining to blueiſh. In a maſs this earth is opaque; but when ſmall fragments of it of a minute thickneſs are examined, they have a ſemi-tranſparency.

In this reſpect it has a ſtrong reſemblance to horn; it is very ſoft, and may be cut with a knife like hard cheeſe. It is more unctuous to the touch, and a little more adheſive to the tongue than the earth of Baudiſſero.

Reſembles horn, cuts like hard cheeſe, more unctuous, and adheres more to the tongue than the firſt.

Treated with the acids, like the ſoft ſpecies of Baudiſſero, it becomes diluted, and then diſſolves, but has however a very remarkable difference, which is that it diſſolves in all the acids without the leaſt effervescence.

Does not effervesce with acids.

It alſo does not yield the leaſt appearance of carbonic acid on expoſing it to the fire in cloſed veſſels furniſhed with ſyphons, which communicate with lime water.

This earth, like that of Baudiſſero, does not contain the leaſt trace of alumine or of oxide of iron.

It contains, ſimilarly to that of Baudiſſero, a little ſulphate of lime and muriate of magneſia, which may be ſeparated by lixiviation in water.

Conſiſts of the ſame ſubſtances as the firſt.

The remainder conſiſts entirely of magneſia and ſilex; but the proportion of this laſt is greater in it than in that of Baudiſſero. It may be computed at from 10 to 20 hundredth parts.

But contains more ſilex.

When this earth is kept in contact with the air its external characters change.

Its colour becomes by degrees a dull white, the ſame as has been remarked of the earth of Baudiſſero.

Changes its appearance on expoſure to the air, to a dull white;

Its ſemi-tranſparence is loſt; its particles ſeparate, and in two or three weeks it is found to have abſorbed carbonic acid to that degree as to make as marked an effervescence with the acid

Loſes its ſemi-tranſparence, and abſorbs carbonic acid, ſo as to effervese with acids, and be-

comes like the earth of Baudiſſero, with the ſmall difference noticed.

acid as the earth of Baudiſſero. In a word, it is completely the ſame as this laſt, with this ſole difference, that phyſically conſidered it is leſs compact, and becomes even friable, and chemically conſidered it contains a little more ſilex.

It appears then to be well proved that the earth of Baudiſſero and that of Caſtellamonte are each a true native magneſia, mixed with a little ſilex. In the earth of Caſtellamonte it is ſufficiently demonſtrated that it contains no carbonic acid when in the boſom of the earth; and that it only contains it when, after a long expoſure to contact with the air, it can abſorb it from the atmoſphere. That of Baudiſſero contains in truth carbonic acid, but the quantity is much inferior to that which it ought neceſſarily to contain to be conſidered a carbonate of magneſia; beſides the earth of Baudiſſero having been worked for a long time, and being thus in contact with the air, it is from the atmoſphere it muſt have drawn it, and that in proportion to the time it has been expoſed; at leaſt I have no doubt that if the earth of Baudiſſero was dug up from a certain depth, no carbonic acid would be found in it.

Earth of Baudiſſero has not acid enough to be carbonate of magneſia.

If dug from a ſufficient depth would probably contain no carbonic acid.

I will conclude this addition to the memoir, by obſerving that the earth of Muſinet at Caſelette, being produced by the decompoſition of the ſame Corneen ſtone or Cacholong, ought alſo probably to be a magneſian earth; but I have not yet made any experiments on this earth; Doctor Bonvoiſin, who has given the analyſis of it in its ſtate of Cacholong and Hydrophane ſtone, propoſes in conjunction with me to repeat the analyſis of this ſtone, in the true ſtate above-mentioned, and in its earthy ſtate; which ſhall be the ſubject of a particular memoir. *

Earth of Caſelette probably magneſian alſo.

The author propoſes to analyſe Cacholong and Hydrophane, and write a memoir of the reſult.

* The laſt uſe which Mr. Giobert mentions for magneſian earth is of the moſt conſequence of the two, for as ſulphate of magneſia is only uſed in medicine, the ſale could not be ſufficiently extenſive to produce much profit on a large ſcale.

The uſe of this earth for pottery is the more deſerving of notice, as it has hitherto been ſuppoſed that argil was alone proper for this purpoſe; and though it was long known that magneſia is of a very refractory nature in the fire, Mr. Giobert ſeems to be the firſt who thought of uſing it in crucibles; which is the more extraordinary, as the *lapis ollaris*, which derives its name from its property of ſerving to make utenſils to bear the fire, is well known to contain a large proportion of magneſia.

VI.

First Communication on an artificial Tan prepared from Coal, chared Wood, resinous Substances, &c. Abridged from the Original of CHARLES HATCHETT, *Esq. F. R. S.**

MR. HATCHETT first notices, that the natural tannin was first extracted from the matters which contain it, by Mr. Deyeux, who considered it as a species of resin; that Mr. Seguin first discovered it to be the substance which in the process of tanning renders animal skins insoluble in water, and imputrescible; but that Mr. Chenevix alone had noticed the effect of heat in giving coffee berries the power in decoction of precipitating gelaten. He then states, that his experiments on lac, and some of the resins having shewed him the powerful action of nitric acid on such substances, induced him to try its effect on asphaltum and jet; these with it formed a dark brown solution, and a precipitate, which by digestion in another portion of the acid became dissolved, and on evaporation produced a yellow viscid substance soluble in water and alcohol, and perfectly similar to that obtained by similar means from the resins, excepting that when burned it had the colour of the fat oils. This result led to the supposition, that the dark brown solution was of the carbonaceous

First discovery of tannin by M. Seguin and Deyeux.

Result of experiments on solution of lac, induced others on asphaltum and jet, with nitric acid.

As native magnesian earth would doubtless be of great use to the potteries of this country, it is a pleasing consideration, that it is extremely probable it may be found in England, as well as on the Continent; for not only steatites and other magnesian stones have already been discovered here, but that salt, which it is M. Giobert's principal object to manufacture, is the natural produce of this country, and therefore the neighbourhood of Epsom, which gives it its name, may well be suspected of containing beds of an earth similar to that of Baudissero.

There is also some reason to suppose, that this earth may be one of those ingredients in china-ware, which the Chinese endeavour to keep secret; indeed it is hardly probable they should be ignorant of its use, in a country, where the finest earthern-ware has been manufactured in the greatest perfection, from periods antecedent to the dates of the authenticated history of Europe, and where of course experiments relative to the composition of this article, must have been varied to the greatest extent.—B.

* Philos. Trans. 1805.

matter

matter, and that the yellow precipitate was the eſſential part of the bitumens, which was confirmed by reſults from amber ; ſeveral experiments were tried with various ſorts of pit-coal, from all which the brown ſolution was obtained in abundance, but thoſe which contained little or no bitumen did not yield the yellow precipitate.

Pit-coal treated in the ſame manner,

Proceſs with the Coal.

100 grains of coal, in each experiment, were digeſted in an open matraſs in a ſand heat, with an ounce of nitric acid (of the ſp. gravity of 1.40) diluted with two ounces of water, which when warm produced efferveſcence, and diſcharged much gas; after two days, a ſecond, and ſometimes a third ounce of the acid was added, and the digeſtion continued for five or ſix days, when nearly the whole was diſſolved, except the precipitate which was conſtantly ſeparated.

and charcoal. Charcoal was next tried, which diſſolved more readily than the pit coal, and left no reſiduum.

The ſeveral ſolutions from aſphaltum, jet, pit coal, and charcoal, were evaporated to dryneſs gradually to prevent burning the reſidue, which from all were of a gloſſy brown ſubſtance, of a reſinous fracture, and had the following properties.

Properties of the reſidua of theſe ſolutions. 1. They were ſpeedily diſſolved by cold water, or alcohol.

2. Their flavour was aſtringent.

3. Expoſed to heat, they ſwelled much, and gave a bulky coal.

4. Their ſolutions in water reddened litmus paper.

5. And gave copiouſly precipitates from muriate of tin, acetite of lead. oxy-ſulphate of iron, of a brown colour, except the tin, which was dark grey.

6. They precipated gold in the metallic ſtate from its ſolution.

7. They alſo precipitated the nitrates of lime, and of barytes, and other earthy ſalts.

8. The fixed alkalis, and ammonia added at firſt, deepened their colour, and afterwards made them turbid.

9. They cauſed precipitates from glue or iſinglaſs ſolutions in water, more or leſs brown according to their ſtrength, which were ſoluble in cold and boiling water, ſo that in their

eſſential

eſſential properties they proved ſimilar to thoſe formed by the varieties of tannin hitherto known, except that they contained no gallic acid or mucilage.

Animal coal from iſinglaſs was alſo tried in the ſame manner, this diſſolved very ſlowly, but left a little of the coal unchanged, its ſolution was of a deeper colour, and managed as the others deſcribed, produced ſimilar effects with the reagents, except ſome difference in the colour of the precipitates; and alſo gave an inſoluble precipitate from the ſolution of iſinglaſs; by which the curious fact is proved, that one portion of the ſkin of an animal may be made to convert another into leather. Reſiduum of animal coal has all the ſame qualities, nearly,

Coak gave a ſolution reſembling that of pit coal, but did not produce the ſame yellow precipitate. and of coak alſo.

Theſe experiments ſhew, that the tanning ſubſtance is beſt procured from carbonaceous matter when it is uncombined with any ſubſtance but oxigen; which was confirmed by experiments on Bovey coal, Suſſex coal, Surturband from Iceland, and deal ſaw-duſt, which being diſſolved in nitric acid, and evaporated, the reſidues diſſolved in water, neither precipitated gelaten, or ſhewed any other ſigns of tanning matter; but when the ſame materials are charred, and treated as before deſcribed, they copiouſly produced the artificial tan; as did alſo teak wood, which Mr. Hatchett had proved to contain neither tannin or gallic acid in its uncharred ſtate. Tanning ſubſtance beſt procured from carbon, uncombined with any thing but oxigen.

Mr. Hatchett had made ſeveral experiments on the ſlow carbonization of vegetable matters in the humid way, principally by ſulphuric acid, occaſionally diluted. Concentrated ſulphuric acid poured on any reſinous ſubſtance reduced to powder, diſſolves it in a few minutes; the ſolution is tranſparent, of a yellow brown colour, and a viſcid oil-like conſiſtence, but after being placed on the ſand bath, grows darker, evolves ſulphuric acid gas, and at laſt becomes a thick liquid of an intenſe black. Carbonization of vegetable matter by ſulphuric acid.

Sulphuric acid of the above ſtrength poured on common turpentine diſſolves it readily, if a portion of the ſolution is then dropped into cold water, a precipitate of common yellow reſin is formed; if after another hour or two, another portion is treated in the ſame way, the reſin produced is of a dark brown, and that thus formed from a ſolution that has ſtood Effects of ſolution of turpentine in ſulphuric acid dropped into water.

five

five or fix hours, is completely black. When the digeſtion is continued for ſeveral days, until no more gas is given out, the reſin will be converted into a black porous coal, which does not contain any reſin, if the experiment has been properly conducted. This coal was about 43 *per cent* of the reſin uſed, and after being expoſed in a platina crucible looſely covered to a red heat, ſtill amounted to 30 *per cent*, and by the flowneſs of its combuſtion, and other circumſtances, approached nearly the character of ſome mineral coals.

Products of this operation diſſolved in nitric acid.

A portion of the coal, the black reſin, brown reſin, and yellow reſin obtained from the turpentine deſcribed, and alſo ſome of the turpentine itſelf, were each diſſolved in nitric acid, and reduced to dryneſs; the reſidua, which varied in colour from yellow to dark brown, according to the ſubſtance employed, were diſſolved in water, and examined with iſinglaſs and other reagents.

Their reſidues after evaporation are tried with gelaten, &c. Effects produced.

The ſolution from the turpentine reſiduum, that of the yellow reſin, and the brown reſin, did not precipitate gelaten.

That from the black reſin yielded a conſiderable portion of the tanning ſubſtance, and that from the coal afforded it in great abundance. Hence it appears, that theſe ſubſtances yielded artificial tan only in proportion to their converſion into carbon.

Various kinds of wood, copal, amber and wax, reduced to coal by ſulphuric acid, yielded ſimilar products on being treated with nitric acid.

Tan formed by alcohol.

Mr. Hatchett formed the artificial tan from the reſins, and gum reſins (ſuch as common reſin, elemi, aſſafœtida, &c.) when reduced to the ſtate of coal from long digeſtion with ſulphuric acid, by means of alcohol, without uſing any nitric acid: In the carbonized ſtate mentioned, they are digeſted in the alcohol, a portion is diſſolved, a dark brown ſolution is formed, which by evaporation yields a maſs ſoluble in water as well as in alcohol, and which precipitates gelaten, acetite of lead, and muriate of tin, but produces only a ſlight effect on oxy-ſulphate of iron.

Suppoſition relative to tan from peat.

The author ſuppoſes, that the tanning matter known to be evolved by peat in certain places, is effected by a proceſs in ſome reſpects ſimilar to the above, ſince if it was produced by its mere digeſtion in water, all peat would afford it, which is contrary to experience.

Mr.

Mr. Hatchett put his discovery to the test of real practice, having actually *converted skins into leather by the artificial tan* procured as described, but observes, that the production of this substance, for the present, must be considered only a curious chemical fact, not altogether unimportant, and not capable of economical application, though he hopes, that hereafter a process may be discovered for preparing this species of tan sufficiently cheap to enable tanners to use it in their business.

Leather made by the artificial tan.

Hope of its economical production.

There is reason to suppose, that it would be superior to common tan for this purpose, as it appears from experiments mentioned in Mr. Hatchett's second paper on the same subject (which will be given in the next number) that "solutions of the artificial tanning substance seem to be completely imputrescible, neither do they ever become mouldy like the infusions of galls, sumach, catechu, &c."*

Reason for thinking it superior to common tan.

VII.

Memoir on the Discovery of a Factitious Puzzolana, presented to the French National Institute, by M. Dodun, *Engineer in Chief of Bridges and Highways in France* †.

THE deposited dust of ancient volcanic substances, has been long used in Flanders, and the adjacent countries, as a substitute for the Italian puzzolana, under the name of *trass*, or *ashes of Tournai*.

Factitious puzzolana of Flanders;

M. Faujas has proved by decisive experiments, made by order of government, that certain lutulent eruptions of ancient volcanoes at Vivarais, had the same qualities as the

that of Faujas.

* The peculiar tanning property of the water of certain peat bogs and morasses, may be otherwise accounted for, than by Mr. Hatchett's supposition, by the fact, that peat is not uniformly the production of the same vegetable substances: wherever heath, tormentil, and perhaps some other plants, are found in abundance, the peat water will have this quality; in the case of heath, at least, it cannot be doubted; and perhaps the peat which does not yield tan may owe this deficiency to the total absence of vegetables of this species.—B.

† *Journal de Physique*, Tom. 61.

puzzolana

that of M. Bagge.

puzzolana of Italy, and might be uſed inſtead of it. M. Bagge, a Swede, is alſo known to have compoſed an artificial puzzolana cement, with a black, hard, and ſlatey ſchiſt; but until 1787, no one ever thought that the territory of France contained in abundance non-volcanic ſubſtances capable of taking place of the Italian puzzolana with economy and advantage.

M. Dodun diſcovered his by chance.

The diſcovery which I here preſent has, like many others of great utility, been the effect of chance.

The habit of examining the nature of ſtone in its bed, which enables the obſerver to judge of its qualities at firſt ſight, fixed my attention on an immenſe quantity of calciform fragments of iron ore, in beds of from eight to ten feet thickneſs, following exactly the parallelism of the ſlightly inclined declivities, in the neighbourhood of Caſtlenaudery. I perceived in the adjacent fields many ſubſtances of the ſame nature ſcattered over the ſurface of the earth, of violet, brown, and black colours, which from their appearance, had a perfect reſemblance to compact lava, which ſeemed extraordinary in a country where there was no appearance of ancient craters, or of volcanic eruptions. Theſe I ſoon found out had been brought to this ſtate by ſerving as hearths, or encloſures to the fires kindled in the fields by the peaſants, either for agricultural purpoſes, or perſonal convenience when they watch their flocks in winter; as I ſaw ſoon after many ſimilar arranged by hand on one another for theſe purpoſes.

Saw great beds of ferrigunous oxides at Caſtelnaudery.

Obſerved burnt fragments in the fields like compact lava,

which ſimilarity made him think them fit for puzzolana.

The ſimilarity of theſe fragments to volcanic products excited my deſire to form a cement from them, by treating them in the ſame manner as puzzolana earth. The great quantity of iron which theſe oxides ſeemed to me to contain, the abundance of their ſiliceous particles, and the alumen which evidently entered into their compoſition, their great weight, and their non-efferveſcence with acids, altogether made me preſume, that the cement formed from them would bind under water, and my expectation was not deceived.

Convinced by long experiments of its ſuperiority to the Italian.

Fifteen months ſucceſſive experiments, to diſcover the proportion of lime which this oxide would abſorb to harden in water, without cracking when in the air, have convinced me, that my factitious puzzolona had all the good properties of that of Italy, without its faults. At this time I determined to propoſe its uſe in the public works, and demanded that comparative experiments ſhould be made between it and

Propoſed for uſe in the public works.

the

the Italian puzzolana, in preſence of the Commiſſaries of the Province of Languedoc, and of the Directors of the canal which joins the two ſeas. Great blocks of *Beton* compoſition made with both cements, were thrown into the reſervoirs adjacent to the lock of Saint Roch, at Caſtelmandery, being firſt plaiſtered over with the reſpective compoſitions

and tried comparatively before the Commiſſaries of Languedoc. In the reſervoir near the lock of St. Roch.

Six months after, the water was drawn off from the bodies of maſonry, and it was then ſeen that the factitious puzzalona had acquired a ſolidity at leaſt equal to that of Italy. The plaiſter made with the Italian puzzalona was cracked and chapped, but that formed from the factitious kind had entirely preſerved the unity of its ſurface.

Its ſuperiority to the Italian ſort.

The ſtates of *Etats* of Languedoc altogether convinced of the authenticity of this diſcovery, by the reſults of the comparative trials of both kinds of cement which they had ſeen, and by the certificates of their commiſſaries, and perſuaded of the great advantage it would be to France, decreed in 1789, in their laſt meeting, that the factitious puzzolana ſhould not only be uſed inſtead of the Italian in the works under their direction; but moreover, that it ſhould be demanded in favour of the author of it, as a teſtimony of public gratitude, that government ſhould authorize the free circulation of it every where.

Teſtimony in its favour from the ſtates of Languedoc.

The great conſumption of this factitious puzzolana obliged me to extend its manufacture, I formed a partnerſhip with the proprietor of the ground. The foundation of an eſtabliſhment on a great ſcale was laid at the mountain itſelf where the materials were found. The works carried on in its vicinity were likely to farther reduce the coſt of the article, which was already one half leſs than that of Italy, and the public were about to enjoy the advantages of this manufacture, when the revolution paralyſed every thing.

Extenſive works of it began.

Stopped by the revolution.

In 1791 I informed the conſtituent aſſembly of this diſcovery; the certificates which proved it, and the reſults of the experiments were depoſited at the office; the matter was ordered to be examined by M. M. Pelletier and Berthollet, and the aſſembly conſidering, that this factitious puzzolana might be of the greateſt uſe to France, decreed that 2000 francs ſhould be granted to its author, which was paid accordingly.

The diſcovery declared to the conſtituent aſſembly.

Approved of and rewarded.

On

On this occaſion the celebrated Mirabeau declared the diſcovery to be ſo valuable, *"that if it had not yet been made, public encouragement ſhould be held out to excite it."*

The troubles of France retards the manufacture of it.

The Conſtituent Aſſembly wiſhed to have numerous ſimilar eſtabliſhments ſet on foot in France, ſo well were they convinced of its national importance; but the misfortunes of the times prevented the execution of a project, which the grand Chief of the empire may eaſily realize, to the advantage of the country, whenever it ſeems good to him to do ſo.

Reſearches on the amelioration of our cements, and particularly on the nature of the materials proper to form artificial puzzolana, led me to try the calcination of various ſchiſts, of the bitumenous, ferruginous, and argillaceous ſorts.

Examination of different ſchiſts.

The black ſlatey ſchiſt of M. Bragge, ſo common in France, was not forgotten: It is almoſt the ſame as that which the elder M. Grathieu eſſayed at Cherbourg laſt year; but I have conſtantly found that theſe ſchiſts always contain too little iron. I perceived that their repulſion of the water was ſlow and feeble, and that their ſolidification in the water was owing to the good quality of the lime.

Contain too little iron for puzzolana.

I was thus obliged to recur to my quartzoſe oxides of iron, from their containing a greater quantity of ferruginous principles; and can aver with the ſkilful Faujas, that the puzzolanas owe their property of hardening in water ſolely to the ferruginous particles which they contain: of this I have had many proofs. This truth is farther demonſtrated in the pudding-ſtones, the breſcias, and generally in all the amygdaloides with a ferruginous baſe or cement.

Puzzolanas owe their qualities to the iron contained.

Theory of cements little advanced.

The theory of our cements is but little advanced; perhaps we take ſimple conjectures for proofs relative to them. We effect the regeneration of ſilex, and of the carbonate of lime; we know the acid gaſes which perform the principal part in the affair: but in this important work we have been long ignorant of the degrees of their reciprocal affinity, their quantity, and the mode of their reſpective combinations. Our knowledge on this matter is confined to a few facts.

Two different preparations of the puzzolana.

Many experiments have proved to me that the puzzolana, which ſooneſt forms a body in the water, is not fit to be employed in the open air, where it cracks and chaps in all directions. And that which is proper for the air, and which acquires

quires and preſerves its tenacity in it, ſets but imperfectly in water. This difficulty, of which the Inſtitute will perceive the cauſe, has obliged me to keep two ſorts of the factitious puzzolana; on the reciprocal uſe of which a memoir of inſtruction will accompany the ſale. The two ſorts may be diſtinguiſhed by their colour.

The factitious puzzolana proper for works under water, is of a reddiſh-brown. That which is fit for works expoſed to the air, is a dark violet. The latter is uſed for terraces, the embankments of baſons, for the compoſition of incloſures, or for light roofs. Bridges of a ſingle arch may be formed with it; and I have ſeen it adhere ſo ſtrongly to glazed tiles, that it was neceſſary to break the tiles to detach it.

One fit for water-works.

Another for expoſure to the air, and proper for terraces, roofs, and arches.

The puzzolana proper for conſtructions beneath the water, forms the moſt ſolid body in it. Three months after immerſion it is an actual ſtone capable of receiving a poliſh. The lime in it is always regenerated into carbonate of lime in ten weeks.

Water puzzolana forms a ſtone capable of a poliſh.

When it may be thought by any one that he has been deceived as to the certainty of theſe effects, it will always be found, that he either has not obſerved the quantities directed of the puzzolana and the lime, or that he has uſed the reverſe of that kind of the cement proper for the work.

Nullity of effect cauſed ſolely by miſtakes of the operator.

I commonly uſed lime in the ſtate of impalpable powder, ſlacked in Lafaye's manner, for works expoſed to the air; and employed lime in the ſtate of *putty*, for works which were to be covered with water. Sometimes I uſed lime in powder for the ſame work. This difference depends on the degree of goodneſs of the lime, on its greater or leſſer richneſs, or its proportional poverty. Cuſtom gives the advantage of knowing the different kinds on mere inſpection.

Lime uſed in powder with it; and in putty.

The uſe of lime in powder appeared to me to merit a preference in the preparation of mortars or cements. I prepared my factitious puzzolana in a certain quantity as ſoon as I knew the proper proportion of the lime; and I had thus the advantage of being able to work it in troughs in the ſame manner as ſulphate of lime. The whole was well mixed together and put into ſacks; by which means the maſons had nothing to do with the mixture of the articles (which is too often left to unprincipled workmen); and being thus maſter of the reſpective

Advantages of getting the materials ready mixed.

ſpective proportions of the puzzolana and the lime, I could always be aſſured of the ſolidity of my cements.

Exterior characters of the iron oxides uſed.

There remains for me to deſcribe the exterior characters of the quartziferous ferruginous oxides, which form the baſis of my factitious puzzolana, and to relate the analyſis of them which I made about 18 years ago. I will content myſelf with offering the comparative reſults with the Italian puzzolana, both in the dry way and the moiſt.

Exterior Characters of the quartziferous Oxides of Iron.

Slight calcination changes them from brown to red.

A greater changes them to a deep brown.

Long continued heat renders them black and porous like lava.

Their colour is of a reddiſh-brown before calcination, or ſlightly violet. A light torrification gives them a clearer red tint or a deep violet: one more intenſe renders them of a deep brown or of a violet-brown inclining to a black. The degree of the calcination for uſe is confined to thoſe two ſtates.

Urged at a longer continued heat, the colour becomes a deep black, then the ſubſtance becomes porous, entirely ſimilar to certain lavas of our modern or ancient volcanos, with which it is then difficult not to confound them.

Their fracture grained and earthy, and they contain quartz cryſtals.

Needles of ſchorl, amphibole, and tourmalines.

Their fracture is grained and a little earthy, and ſmall cryſtals of quartz may be diſtinguiſhed in them by the naked eye, and almoſt always angular fragments of gray or milky quartz; a powerful magnifying lens cauſes in ſome fragments the diſcovery of needles of ſchorl, the amphibole of Hauy, and ſome ſmall tourmalines.

Their ſmell argillaceous.

Their ſmell is ſtrongly argillaceous on breathing on them with the mouth.

Give no ſparks.

There is no fire produced by the uſe of the ſteel, when it does not ſtrike a quartoſe particle.

Do not effervеſce.

They do not effervеſce with acids either cold or hot.

Are affected by the magnet.

The magnet acts a little on theſe oxides before calcination, and ſtrongly, or perceptibly, after it.

Weight of a cubic foot.

The medium weight of a cubic foot is 125°; that of the Italian puzzolana is but 91°.

Analyſis by the moiſt Way.

Analyſis in the moiſt way of the iron oxides,

I ſhall not weary the aſſembly by a detail of the manipulations relative to the ſolvents and re-agents which art uſes for the decompoſition of bodies, and ſhall only ſay, that ſilex, iron, alumen, and a ſmall portion of manganeſe, are the conſtituent parts of theſe oxides.

I repeated

I repeated theſe experiments many times, and had for a medium reſult from an hundred pounds, chemical weight,

50 parts of ſilex;
31 —— of iron;
16 —— of alumen;
3 —— of manganeſe, and loſs.

100

If this analyſis be compared with that of the puzzolana of Italy, which contains in 100 parts— and of the Italian puzzolana.

50 of ſilex;
25 of alumen;
16 of iron;
3 of lime;
6 of loſs;

100

their reſpective properties may be appreciated according to the proportions of their integrant parts.

The exceſs of alumen cauſes the plaiſters made from the Italian puzzolana to crack and chap in the open air: this fault ariſes from their great oxidation. I have been able to replace in them thoſe principles which they loſt by decompoſition. Exceſs of alumen cauſes the Italian kind to crack.

Analyſis in the dry Way.

I endeavoured to obtain a regulus from theſe oxides of iron by uſing a violent heat. I followed the proceſs of Kirwan for the fuſion of ſiliceous and argillaceous ores of iron; yet I never obtained a ſingle metallic button; and only found at the bottom of the crucible a vitrified maſs of an opaque black, or a ſcoria in the ſtate of crude caſt iron. Analyſis in the dry way by Mr. Kirwan's proceſſes; afforded no metal.

Deſirous to know if I could procure a malleable button by uſing the blowpipe, taking borax for the flux and ſupporting the oxide on charcoal; I ſtill could only obtain a ſpongy ingot reſembling crude caſt iron, and breaking both when hot and when cold. The blowpipe tried unſucceſsfully.

Being placed on a ſupport of glaſs (according to my method publiſhed in the Journal de Phyſique, Tome XXXI. pages 116 and 139), the oxide fuſed at the ſecond attempt, the ſup- The oxide fuſed on glaſs.

port was coloured green, and ſmall grains of iron were ſeen to paſs firſt of a dark-green colour, then of a bright green, and afterwards to diſappear in evaporating. There remained on the globule only a ſlight tinge of blackiſh-green.

General reſult.

The reſult of all theſe facts ſeems to be, that this oxide is entirely deprived of its metallic principle, and that its ſuper-oxigenation renders it reducible and refractory.

The oxide may be uſed as a pigment.

The arts may draw ſome advantage from theſe oxides, by uſing them in pigments for buildings. I ſucceeded after many waſhings, in extracting from them a beautiful brown-red colour equal to that of commerce, and applied it to uſe ſucceſsfully.*

* This paper has been abridged in its introduction, in the details relative to negociations with the Conſtituent Aſſembly, and in ſome other points a little irrelevant to the puzzolana; but all matters directly tending to illuſtrate its nature and properties have been carefully copied.

M. Dodun's diſcovery may be of ſome uſe to this country, as there are in many parts of it large maſſes of iron-ſtone, and ſome is found in the vicinity of moſt coal-mines.

It has been long known that iron ochres have the ſame property of forming puzzolana with lime, when properly roaſted, and this circumſtance is mentioned at large in Chaptal's Chemiſtry. A patent has alſo been obtained in this country for the application of iron pyrites to the ſame purpoſe, the right to which was purchaſed long ago by Mr. Samuel Wyat. But the novelty of M. Dodun's diſcovery is, that poor iron-ſtone is equally fit for this uſe, as the other ſubſtances mentioned, which is of the more importance as it is very plentiful, and may often be procured in ſituations where the others cannot.

It may not be amiſs to mention here, that baſalt treated in the ſame manner, has the ſame property as the puzzolana: the whin-ſtone, of which the ovoidal paving-ſtones conſiſt moſtly, is of this kind; and it is found in great abundance in theſe countries, in different forms.—B.

Experiments

VIII.

Experiments and Obſervations upon the Contraction of Water by Heat at low Temperatures. By THOMAS CHARLES HOPE, *M. D. F. R. S. Ed. Profeſſor of Chemiſtry in the Univerſity of Edinburgh. From the Edinburgh Philoſophical Tranſactions, for* 1804.

Expanſion of water by cold, &c. near the freezing point.

TO the general law, that bodies are expanded by heat, and contracted by cold, water at the point of congelation, and for ſome degrees of temperature above it, it ſeems to afford a very ſingular and curious exception.

The circumſtances of this remarkable anomaly have been for ſome time believed to be the following:

General or uſual ſtatement of the facts.

Ice-cold water contracts by increaſe of temp. to about 42°, and then expands;

When heat is applied to water ice cold, or at a temperature not far diſtant, it cauſes a diminution in the bulk of the fluid. The water contracts, and continues to contract, with the augmentation of temperature, till it reaches the 40th or 41ſt degree. Between this point and the 42d or 43d, it ſuffers ſcarcely any perceptible change; but when heated beyond the laſt-mentioned degree, it begins to expand, and increaſes in volume with every ſubſequent riſe of temperature.

and ſuffers the reverſe change by cooling.

During the abſtraction of caloric, the peculiarity in the conſtitution of water equally appears. Warm water, as it cools, ſhrinks, as other bodies do, till it arrives at the temperature of 43° or 42°. It then ſuffers a loſs of two degrees without any alteration of denſity. But when farther cooled, it begins to dilate, and continues to dilate, as the temperature falls, till congelation actually commences, whether this occurs as ſoon as the water reaches the 32°, or after it has deſcended any number of degrees below it.

This ſuppoſed peculiarity has

Suppoſing this peculiarity of water to be eſtabliſhed, it muſt appear, indeed, a very odd circumſtance, that heat ſhould produce contraction in this fluid, while it cauſes expanſion in other bodies *; and no leſs ſtrange, that within one range of tempera-

* Is this mode of change peculiar to water?—I do not know of any experiments with other fluids, except that mentioned on page 343. Perhaps it may be common to all, or at leaſt to all thoſe which expand by congelation. Deciſive trials of this point are the more deſirable, becauſe ſome of Count Rumford's general inductions require or ſuppoſe that ſeawater ſhould not be thus affected.—N.

ture it ſhould contract, and in another expand, the very ſame ſubſtance. Before a deviation from ſo general a law ſhould be received as matter of fact, the proofs of its exiſtence ought to be clear and indiſputable.

been hitherto deduced from experiments in narrow necked veſſels,

The experiments hitherto publiſhed, from which this ſingularity has been deduced, have all of them been performed upon water contained in inſtruments ſhaped like a thermometer glaſs, and conſiſting of a ball with a ſlender ſtem; and the expanſive or contractive effects of heat and cold have been inferred, from the aſcent or deſcent of the fluid in the ſtem.

of which the capacities alſo vary by change of temperature.

To ſuch experiments it has been objected, that the dimenſions and capacity of the inſtrument undergo ſo much change, from variation of temperature, that it is difficult, if not impoſſible, to determine how much of the apparent anomaly ought to be imputed to ſuch changes, and that it is not improbable that the whole of it may be aſcribed to them.

The author ſhows the effect by other means.

The object of this paper, which I have now the honour to read to the ſociety, is to prove by a ſet of experiments, conducted in a manner altogether different, that the common opinion is founded in truth, and that water preſents itſelf as that ſtrange and unaccountable anomaly which I have already deſcribed.

Previous hiſtory.

It is worth while, before detailing my experiments, to give a ſhort account of thoſe obſervations which led to the diſcovery of the fact, and which in ſucceſſion have extended our knowledge of it, as well as of thoſe obſervations which have at different periods been offered to diſcredit, and to bring it into doubt.

Dr. Croune firſt obſerved that water appears to expand before it freezes.

The firſt obſervation relative to this ſubject was made by Dr. Croune, towards the cloſe of the 17th century, while engaged in inveſtigating the phenomena of the great and forcible, though familiar, expanſion which happens to water at the inſtant of freezing; a matter which occupied in a conſiderable degree, the attention of his fellow-members of the Royal Society of London in the earlier years of that inſtitution.

His narrative. The experiment ſhewed that water roſe in a long necked veſſel by cooling:

I ſhall relate in his own words his firſt obſervation: "I filled a ſtrong bolt-head about half-way up the ſtem with water, a day or two before the great froſt went off, marking the place where the water ſtood; and placing it in the ſnow on my leads, while I went to put ſome ſalt to the ſnow, I found it above the

the mark ſo ſoon, that I thought the mark had ſlipt down, which I preſently raiſed to the water, and as ſoon as ever I mixed the ſalt with the ſnow, the water roſe very faſt, about one-half inch above it. I took up then the glaſs, and found the water all fluid ſtill: it was again ſet down in the ſalt and ſnow; but when I came about an hour after to view it, the ball was broke, and the water turned to hard ice, both in the ball and ſtem *."

From this experiment Dr. Croune drew the concluſion, that water, when ſubjected to cold, actually began to expand before it began to freeze. On announcing it, however, to the Royal Society, on the 6th of February 1683, Dr. Hooke immediately expreſſed ſtrong doubts, and aſcribed the aſcent of the water in the neck of the veſſel to the ſhrinking of the glaſs occaſioned by the cold.

Whence he inferred an actual expanſion. But Dr. Hooke aſcribed the effect to the veſſel.

To obviate this objection, and to preclude, as far as was poſſible, the influence of the change of capacity in the apparatus from an alteration of its temperature, a bolt-head was immerſed in a mixture of ſalt and ſnow, and into it, when cooled, was poured, to a certain height, water previouſly brought to near the freezing point. The water began inſtantly to riſe as before, and when it had aſcended about one-fourth of an inch in the ſtem, the veſſel was taken out, the whole water remaining fluid.

Dr. C. repeated his exp. with the ſame event, in a veſſel previouſly cooled;

Theſe experiments, ſupported by others of a ſimilar nature, communicated by Dr. Slare to the Society on the 20th of the ſame month, appear to have ſatisfied its members, in general, of this fact, that water, when on the point of congealing, and while ſtill fluid, is actually ſomewhat dilated previous to the remarkable expanſion which accompanies its converſion into ice.

which gave ſatisfaction;

Dr. Hooke, however, continued unſhaken, and retained the doubts he had expreſſed.

but not to Dr. Hooke.

Remarkable as the fact, as now ſtated, muſt have appeared, it ſeems not to have excited particular attention, nor to have ſolicited more minute examination; and indeed though philoſophers did not loſe ſight of it, yet for near a century no one inveſtigated it more carefully. Mairan, in his treatiſe on ice in 1749, and Du Creſt in his diſſertation on thermo-

* Birch's *Hiſtory of the Royal Society*, Vol. iv. p. 263.

meters

Modern experiments of De Luc.

meters in 1757, appear to be well aware of this property of water, but it is to M. De Luc that we owe the knowledge of the leading and more intereſting circumſtances, (*vide Recherches*, &c. 1772.)

Having devoted his attention to the examination and improvement of the thermometer, he was naturally led to the inveſtigation, while engaged in aſcertaining the phenomena of the expanſion and contraction of different fluids by heat and cold.

He uſed thermometer glaſſes, and found the water to deſcend by cooling till 41°, and then riſe till freezing.

He employed in his experiments thermometer glaſſes; and the included water, at or near the term of liquefaction, deſcended in the ſtem, and appeared to him to ſuffer a diminution of bulk by every increaſe of temperature, till it arrived at the 41ſt degree. From this point its volume increaſed with its temperature, and it aſcended in the tube. This fluid, when heated and allowed to cool, ſeemed to him to contract in the ordinary way, till its temperature ſunk to the 41°, but to expand and increaſe in volume, as the temperature fell to the freezing point.

The denſity of water, he thence inferred, is at its maximum at 41°, and decreaſes with equal certainty whether the temperature is elevated or depreſſed.

So that its denſity at 50° and at 32° appears the ſame.

M. de Luc ſays, indeed, that very nearly the ſame alteration in volume is occaſioned in water of temperature 41°, by a variation of any given number of degrees of temperature, whether they be of increaſe or of diminution; and conſequently that the denſity of water at temperature 50, and at temperature 32°, is the ſame.

His theory.

This philoſopher did not conceive that the conſtitution of water, in relation to caloric, undergoes a change at the temperature of 41°, ſuch that ſhort of this degree caloric ſhould occaſion contraction, and beyond it expanſion. He imagined that heat in all temperatures tends to produce two but quite oppoſite effects on this fluid, the one expanſion, the other contraction.

In low temperatures, the contractive effects ſurpaſs the expanſive, and contraction is the conſequence: In temperatures beyond 41°, the expanſive predominate, and the viſible expanſion is the exceſs of the expanſive operation over the contractive.

In

In 1788, Sir Charles Blagden added the curious obſervations, that water, which by ſlow and undiſturbed refrigeration permits its temperature to fall many degrees below its freezing point, perſeveres in expanding gradually as the temperature declines; and that water having ſome muriate of ſoda or ſea-ſalt diſſolved in it, begins to expand about the ſame number of degrees above its own term of congelation that the expanſion of pure water precedes its freezing, that is, between eight and nine degrees. More lately, (*Philoſophical Tranſactions*, 1801), he, or rather Mr. Gilpin by his direction endeavoured to aſcertain, by the balance and weighing bottle, the amount of this change of denſity cauſed by a few degrees of temperature.

Sir Cha. Blagden's obſ. that water may be cooled many degrees below 32° without freezing, and continues to expand.

Every one muſt be familiar with the uſe which Count Rumford has made of this peculiarity in the conſtitution of water, in explaining many curious appearances that preſented themſelves in his experiments upon the conducting power of fluids, and in accountiug for certain remarkable natural occurrences. The Count, with his uſual ingenuity, has endeavoured to point out the important purpoſes which this peculiarity ſerves in the economy of nature, and to aſſign the final cauſe of ſo remarkable an exception from a general law.

Count Rumford's applications of this doctrine to the œconomy of nature.

In recording the obſervations and opinions that have been publiſhed concerning this point, I might now, in order, notice thoſe of Mr. Dalton of Mancheſter, related in the fifth volume of the *Mancheſter Memoirs*, which tended to confirm and enlarge our knowledge of it. But as Mr. Dalton himſelf has called in queſtion the accuracy of the concluſion which had been drawn from his experiments, and from thoſe of preceding obſervers, I ſhall only remark, that they are of the ſame nature, and nearly to the ſame purport, as thoſe of M. de Luc.

Mr. Dalton's experiments,

It was in conſequence of a communication with which Mr. Dalton favoured me, three months ago, that my attention was directed to this ſubject. He informed me, that after a long train of experiments he was led to believe that he, and his predeceſſors in the ſame field of inveſtigation, had fallen into a miſtake with regard to the contraction of water by heat, and its expanſion by cold, in conſequence of overlooking or underrating the effect which the change in the capacity of the thermometer-ſhaped apparatus employed, muſt occaſion on the apparent volume of the fluid. He ſtated, in general terms, that

who queſtions the truth of the concluſions,

because the point of greateſt apparent denſity is different in different veſſels; viz. in earthen-ware at 34°, glaſs 42°, braſs 46°, and lead 50°.

that on ſubjecting water to different degrees of temperature, in inſtruments made of different materials, he found the point of greateſt denſity was indicated at a different temperature in each.

In an apparatus, having a ball of earthen-ware, it was at the 34th degree; of glaſs at the 42d; of braſs at the 46th; and of lead at the 50th. And as water could not follow a different law, according to the nature of the ſubſtance of the inſtrument, he conceived that the appearance of anomaly in this fluid originated entirely in the containing veſſel, which muſt cauſe the fluid in the ſtem to fall or riſe according as its expanſions are greater or leſs than thoſe of the included liquor.

A detail of theſe important experiments has, ere now, been tranſmitted for publication in the *Journals of the Royal Inſtitution of London* *.

Mr. Dalton ſupports Dr. Hooke.

I have already noticed that Dr. Hooke endeavoured to explain in the ſame manner the original experiment of Dr. Croune. This explanation apparently gathers much force from theſe experiments of Mr. Dalton.

De Luc and

It is proper, however, to ſtate, that M. de Luc was perfectly aware of the alteration in the dimenſions of his glaſs apparatus, but deemed the change too trifling to have any material influence.

Blagden were attentive to the veſſel:

Sir Charles Blagden paid greater attention to the circumſtance, and by calculation attempted to appreciate what allowance ought to be made for the change of capacity in the amount of the apparent changes of volume.

When it is conſidered, that the whole amount of the apparent change is but very ſmall, and that the expanſibility of the glaſs is with difficulty aſcertained, and is variable by reaſon of the fluctuating proportions of its heterogeneous conſtituents, it muſt be acknowledged, that preciſion in ſuch a calculation cannot poſſibly be attained, and can ſcarcely be approached. On this account, all the experiments already noticed are open to the explanation of Dr. Hooke, and in ſome meaſure liable to the objection which he had urged. I confeſs, that the experiments of Mr. Dalton, in perfect concurrence with that explanation,

and various reaſons afford ground for doubt.

* They were tranſmitted to our Journal by the author in Vol. X. page 93.

created

created conſiderable doubts reſpecting the exiſtence of the peculiarity of water; againſt the probability of which circumſtance, all analogical reaſoning, and every argument *à priori*, ſtrongly militate.

Unwilling to remain in uncertainty, and conſidering it as a point of much curioſity and intereſt, I have endeavoured to inveſtigate the ſubject by experiments conducted in a totally different manner, equally calculated to exhibit the ſingular truth, but free from the objections to which the others are liable. In them, it was my object to provide, that neither the changes of the actual volume of the water, nor the alterations in the dimenſions of the inſtrument, ſhould have any influence whatever.

The author's experiments were not made uncertain by the cauſes before ſtated.

I have already taken occaſion to ſtate, that the purpoſe of this paper is to prove, by experiments on the principle now mentioned, that in the conſtitution of water there really exiſts the ſingularity often noticed.

I ſhall firſt ſtate the plan of the experiments, and then detail the particulars of the moſt remarkable of them.

When any body is dilated, whether by heat or cold, it neceſſarily becomes leſs denſe, or ſpecifically lighter; and the oppoſite effects reſult from contraction. This is the circumſtance, as every one knows, which cauſes various movements among the particles of fluids, when any inequality of temperature prevails in the maſs; hence theſe particles are little acquainted with a ſtate of reſt.

His attention was directed to ſhew whether water riſes or ſinks by the preceding changes of temperature;

If a partial application or ſubtraction of heat produce an inequality of denſity in a maſs of fluid, the lighter parts riſe to the ſurface, or the denſer fall to the bottom.

It readily occurred, that I might avail to myſelf of theſe movements, and upon ſtatical principles determine the queſtion in diſpute.

I had only to examine attentively water, as it was heated or cooled in a jar, and to obſerve, by means of thermometers, what ſituation the warmer, and what the cooler parts of this fluid affected.

which could be done by thermometers duly placed.

If I ſhould find that ice-cold water, in acquiring temperature, ſhowed, in its whole progreſs, the warmer parts near the top, it would indicate that water follows the uſual law, and is expanded like other bodies by heat.

Or if I ſhould obſerve that warm water, in cooling to the freezing point, had the coldeſt portion uniformly at the bottom, the

For the cold portions of water during

change of temperature would conſtantly be at the bottom, if denſeſt, throughout ſuch change.

the ſame concluſion would follow; while a different inference, and the exiſtence of the ſuppoſed anomaly, would be deducible ſhould the event prove different. The only circumſtance, I can figure to myſelf as tending in any meaſure to render this mode of examining the point doubtful, is, that water near its congealing point may have ſo little change of denſity occaſioned by a ſmall variation of temperature, that its particles may be prevented by their inertia, or by the tenacity of the circumfluent maſs, from aſſuming that ſituation which their ſpecific gravity would allot to them.

It will appear, however, very clear, from the circumſtances of the experiments which I ſhall immediately detail, that no obſtacle to the ſucceſs and preciſion of the experiments proceeded from this ſource.

It is not neceſſary for me to relate all the experiments I have made. I ſhall reſtrict myſelf to the detail of ſix, which preſent varieties in the modes of procedure, and which afford the moſt ſtriking reſults.

Exp. 1. Ice-cold water expoſed to a warm atmoſphere, was warmer ($1\frac{1}{2}^{\circ}$) below till 38°, after which it was warmer at top.

Exp. I. I filled a cylindrical jar of glaſs $8\frac{1}{2}$ inches deep, and $4\frac{1}{2}$ in diameter, with water of temperature 32°, and placed it on a table, interpoſing a conſiderable thickneſs of matter poſſeſſed of little power of conducting heat. I ſuſpended two thermometers in the fluid, nearly in the axis of the jar, one with its ball about half an inch from the bottom, the other at the ſame diſtance below the ſurface. The jar was freely expoſed to the air of the room, the temperature of which was from 60° to 62°.

The experiment commenced at noon:

	Top Thermom.		Bottom do.
	32°	-	32°
In 10 minutes, -	33+	-	34+
— 30 ——— -	35.5	-	37
— 50 ——— -	37	-	38+
— an hour, -	38	-	38+
— ——— and 10 minutes,	42	-	38.25
— ——— —- 30 ———	44	-	40
— ——— —- 50 ———	46+	-	41+
— 2 hours and 10 minutes,	48	-	42.5
— ——— —- 30 ———	50	-	44
— ——— —- 50 ———	50.5	-	45
— 4 hours, -	54	-	49

Confiding

Confiding in the indications of the thermometers, from this experiment we learn, that when heat flows on all fides from the ambient air into a column of ice-cold water, the warmer portions of the fluid actually defcend, and take poffeffion of the bottom of the veffel.

This downward courfe proclaims an increafed denfity, and teftifies that the cold water is contracted by heat. As foon, however, as the fluid at the bottom exhibits a temperature of 38°, this courfe is retarded and foon ftopped, and with the rife of temperature beyond 40° is totally changed; for when the mafs attains this degree, the experiment equally fhows, that the warmer fluid afcends and occupies the fummit, by its route announcing its diminifhed denfity, and proving that water is now expanded by heat.

Whence it is concluded that the colder (upper) fluid was rarer at the temperatures near freezing; and the warmer (upper) fluid was rarer at the higher part of the fcale.

Exp. II. I filled the fame jar with water of temperature 53°; and that I might obferve the phenomena of cooling, I placed it in the axis of a much larger cylindrical veffel, nearly full of water, of temperature 41°, and, by an earthen-ware fupport, raifed it about three inches from the bottom, taking care that the water fhould be on the fame level in both veffels. As foon as I had adjufted the two thermometers, as in the former experiment, I obferved that the top of the fluid was ftill at 53°, but the bottom had fallen to 49°.

Exp. 2. Water at 53° was every where cooled by enveloping the veffel with ice-cold water. It was warmer at top till 42°, after which it was warmer (4°+) below.

	Top.		Bottom.
In 9 minutes,	52°	-	45
— 15 ——	52	-	44

Now, to accelerate the cooling, I withdrew by a fyphon the water from the large cylinder, and fupplied its place by ice-cold water, mixed with fragments of ice, which by repeated cautious agitation was kept uniformly at the temperature of 32°.

In 23 minutes,	48°	-	42+
— 38 ——	44	-	40
— 43 ——	42	-	40
— 46 ——	40	-	40
— 52 ——	36	-	40
— 58 ——	35—	-	39
— 65 ——	34	-	37
— 75 ——	34	-	36
— 103 ——	34	-	34

This

This experiment is the counterpart of the foregoing, and from the teſtimony of the ſame inſtruments, it appears, that when a cylinder of water of 53° is cooled by circumfluent iced fluid, the colder part of the water takes poſſeſſion of the bottom of the veſſel, ſo as to eſtabliſh a difference of temperature from the ſurface, amounting ſometimes to 8°. And that as ſoon as the fluid at the bottom arrives at the 40th degree, the temperature of the fluid in that ſituation is ſtationary till the ſurface reaches the ſame point.

Whence the ſame concluſion is deduced as in the former experiment.

During the ſubſequent refrigeration, the progreſs of the cooling undergoes a total change. The thermometers tell that the colder fluid riſes to the ſurface; ſo that the top gets the ſtart of the bottom ſoon by 4°, and attains the loweſt temperature of 34° very long before the other falls to the ſame degree.

Theſe circumſtances, I think, lead to the concluſion, that by the loſs of caloric, water at 53° is contracted and rendered ſpecifically heavier, and that this continues to happen till the water come to the temperature of 40°, at which period an oppoſite effect is produced; for now the water, as it cools, becomes ſpecifically lighter, or is expanded.

In this, as well as the former experiment, the complete change in the ſituation, which the warmer and colder parts of the fluid affected, in the progreſs both of the heating and cooling, while every external circumſtance of the proceſs continued unaltered, is particularly worthy of remark.

Exp. 3. A tall jar, nearly 18 inches high, containing water at 50°, was cooled round its *upper part* by ice and ſalt. The temperature fell quickeſt at bottom, till 40°, where it continued ſtationary; after which the ſurface ſunk to freezing, and congealed.

Exp. III. I took a glaſs jar, 17.8 inches deep, and 4.5 in diameter internal meaſure, having a neck and tubulature very near the bottom. I provided alſo a cylindrical baſon of tinned iron, 4.8 inches deep, and 10 inches in diameter, with a circular hole in the middle of the bottom, large enough to receive the top of the jar. By means of a collar and cement I ſecured this baſon, ſo that it encircled the upper part of the jar.

The object of the contrivance was to have the means of applying a cooling medium to the ſuperior portion of a cylinder of water, and it anſwered the purpoſe completely. I introduced the ball of a thermometer through the tubulature, till the extremity of it nearly reached the axis at three-fourths of an inch above the riſing of the bottom, and having fixed it in this ſituation, I rendered the aperture water-tight, by a perforated cork and lute.

This

This very tall jar was placed on a table, with the interpoſition of ſome folds of thick paper, in a room without a fire, of the temperature 42°.

I filled it with water of 50°, and poured into the baſon, which embraced the top, a mixture of powdered ice and ſalt.

From time to time I explored the temperature near the ſurface, by inſerting the bulb of a thermometer to the depth of half an inch nearly in the axis.

The experiment laſted 50 hours.

	Bottom.	Top.	Air.	
One o'clock,	50°	50°	42°	
In 11 minutes,	46.+	—		
— 15 ———	45	48		
— 21 ———	44—	46—		
— 31 ———	42	44		
— 41 ———	41	42		
— 51 ———	40 +	34		At this time a thin film of ice began to form in contact with the glaſs.
— 1 hour 6 min.	40	34		
— ——— 20 ——	39.5			
— ——— 44 ——	39.5			
— 4½ hours,	39.5			A cruſt of ice of ſome thickneſs now lined the glaſs, and air had fallen to 40°.
— 5½ hours,	39			
— 11 hours, *i. e.* at midnight,	39			Cruſt of ice complete.
— 19 hours, *i. e.* next morning,	39			Air 40°.
— 26 hours,	40			Air 40°. So much ice had melted that the cake was detached from the ſide of the veſſel, and floated.
— 32 ——	40			
— 41 ——	40			Air 41°. Ice not all melted.
— 50 ——	41			Air 42°. Ice not entirely gone.

Review of the facts and remarks.

This long protracted experiment preſents ſome ſtriking facts, and its general import, with regard to the ſubject of inveſtigation, agrees with the preceding. In it we ſee, that when the frigorific mixture abſtracted caloric from the upper extremity of a cylinder of water, nearly 18 inches long, and at 50°, the reduction of temperature appeared ſooner, and advanced quicker, at its lower extremity than in the axis at the top, not two and a

half inches diſtant from the cooling power. No one can entertain a doubt that this is owing to a current of cooled and condenſed fluid deſcending, and a correſponding one of a warmer temperature aſcending. Now, if water obſerved the ſame law that other bodies do, and had no peculiarity of conſtitution, the ſame progreſs of cooling ſhould continue. This, however, the experiment teaches us, is not the caſe: as ſoon as the fluid at the bottom exhibits a temperature of 40°, it ceaſes. The colder fluid remains at top, and quickly loſing temperature, ere long begins to freeze. The continuance of the colder fluid at the ſurface ſurely denotes, that it is not more denſe than the ſubjacent warmer water. The legitimate inference from this is, that water of temperature 40° is not contracted by being cooled to 32°.

As the fluid below 40° continued at top, it was not *denſer* than that at 40°.

Did water obſerve the uſual law, and loſe volume along with temperature, this experiment, by its long duration, afforded ample time for the manifeſtation of it.

For not leſs than two days did ice-cold water maintain poſſeſſion of the top, and for the ſame period the temperature at the bottom never fell below 39°. No current, therefore, of cold and condenſed fluid moved from the ſurface, to affect the inferior thermometer, or to atteſt the contraction of water by cold.

Yet the experiment does not ſhow that it was *rarer*.

This experiment, however, I muſt remark, does not warrant the concluſion, that the water is actually expanded, though it in no degree oppoſes it. It proves no more, than that the contraction ceaſes at 40°; and that water of 32° is not more denſe than of 39° or 40°. Nay, ſome may perchance alledge, that it does not prove ſo much; conceiving, that if at 40° the contraction, without ceaſing altogether, becomes very inconſiderable, the difference of denſity occaſioned by the ſubſequent reduction of temperature may be ſo very trifling, as not to enable the cold particles to take that ſituation which their gravity aſſigns to them, in oppoſition to the inertia and tenacity of the ſubjacent maſs; and therefore that the colder, though heavier fluid, may be conſtrained to remain above. That this allegation ſhould have no weight attached to it, the circumſtances of the ſucceeding experiment will clearly ſhow, as I ſhall ſoon notice.

It might be even alledged that a ſmall exceſs of denſity prevailed; but this is not entitled to regard.

That heat which paſſed by direct communication

Before quitting the conſideration of the preſent experiment, it may be worth while to remark, that it may ſeem rather ſurpriſing,

priſing, that the bottom of the fluid was not apparently affected in its temperature by the ice which ſo long occupied its ſurface. It might be expected, though no cold currents deſcended from above, that the caloric ſhould be conducted from below, and that the temperature ſhould by that have been reduced *. I ſuppoſe that the caloric did paſs from the lower ſtrata upwards, but extremely ſlow, by reaſon that fluids, as Count Rumford taught us, are exceſſively bad conductors of heat, and ſo very ſlowly, that the caloric entered from the atmoſphere with ſufficient quickneſs to prevent any depreſſion of temperature below the 39th degree.

muſt have been very ſlowly tranſmitted.

This experiment, I may conclude with remarking, is very well calculated to exhibit the error of the popular opinion, that "heat has a tendency to aſcend."

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* ANNOTATION, BY THE AUTHOR, †

This experiment may perhaps be thought to give countenance to the opinion of the very ingenious Count Rumford, that fluids cannot conduct heat, and that no interchange of heat can take place between the particles of bodies in a fluid ſtate, ſeeing that for two days the fluid at the bottom of the veſſel never fell below 39°, though the ſurface was at 32°.

The opinion of Count Rumford that fluids cannot conduct heat from particle to particle,

From the circumſtances detailed in his ſeventh eſſay, the Count concluded, that heat cannot deſcend in a fluid. From the preſent, it might with equal juſtice be inferred, that heat cannot aſcend.

Had I not the fulleſt conviction that this celebrated philoſopher has puſhed his ideas too far, I might be diſpoſed to conſider this experiment as according well with the hypotheſis.

appears to be inaccurate.

Soon after the intereſting ſpeculations of the Count appeared, I began to inveſtigate the ſubject; and, by a pretty long train of experiments, which I have annually taken an opportunity of detailing in my lectures, ſatisfied myſelf that he aſſigned to fluidity a character that does not belong to it. Though ſince the date of theſe experiments, the public has

† As this note ſubjoined at the foot of the page after the words *temperature ſhould by that have been reduced*, in the original, is of ſuch conſiderable length, I have taken the liberty of putting it in the ſame type as the text.—N.

become

become poſſeſſed of ſeveral ſeries, well deviſed, and, in my opinion, of themſelves concluſive, it may yet be worth while to ſtate the tenor and reſult of them, by which the value of their teſtimony in favour of the conducting power of liquids may be eſtimated.

Experiments to ſhew that heat can deſcend in fluids:

The experiments were of two deſcriptions.

The one ſet, of the ſame nature nearly with thoſe of Count Rumford, was deſigned to examine, Whether heat, when applied to the ſurface, can deſcend in a fluid; and the other to diſcover, Whether, on the mixture of different portions of fluid at different temperatures, an interchange of caloric takes place between the particles;—Water, oil and mercury, having been the ſubjects of the Count's experiments, were employed for the firſt ſet.

To water (and to oil) the heat was communicated from the bottom of a metallic veſſel (in contact with the ſurface of the fluid, and) heated by boiling water within.

To explore the conducting power of water and oil, the apparatus which I uſed conſiſted of two veſſels of tinned iron, both cylindrical, and the one ſomewhat larger than the other. The larger had a diameter of eleven inches, and into it were poured the ſubjects of the trial, to different depths on different occaſions. The ſmaller was ten and a half inches in diameter. By three hooks it was ſuſpended within the larger pan, in ſuch a manner, that the bottom of it exactly reached and came in contact with the ſurface of the fluid. This ſmaller veſſel became the ſource of the heat, by being filled with boiling hot water. The water was changed frequently, care being taken to avoid, by the uſe of a ſyphon, all agitation and diſturbance.

This hot veſſel did not touch that which contained the fluid under experiment; and the containing veſſel was kept cold round the ſurface of the fluid, and therefore did not carry any heat downwards.

In experiments of this nature, the difficulty is to prevent the conveyance of caloric by the ſides of the veſſel. I attempted, and, I think, I ſucceeded, in overcoming this difficulty, by encircling the larger veſſel, as a height exactly correſponding with that of the ſurface of the fluid within, with a gutter or channel about half an inch in depth; and by cauſing a ſtream of cold water to flow conſtantly through a ſyphon into this gutter, while from the oppoſite ſide it ran off by a ſmall ſpout.

The water was ſeveral degrees colder than the ſubject of the experiment; and keeping cool the portion of the veſſel with which it was in contact, it intercepted the heat that would otherwiſe have travelled by this route to the bottom.

Mercury was tried in glaſs veſſels.

For mercury I had recourſe to veſſels of glaſs.

In

In all the experiments a thermometer bore teſtimony that the caloric deſcended from the ſurface to the bottom of the fluid, and demonſtrated, at leaſt to my conviction, that fluids can conduct heat.

In all the experiments the heat deſcended

The progreſs of the heat, however, was very ſlow, and atteſted the important fact, for which we ought to be thankful to the Count—That fluids are very bad conductors.

ſlowly.

The ſecond ſet of experiments was calculated to examine, in a very different manner, the poſition, That all interchange and communication of heat between the particles of fluids is impoſſible.

When a hot and a cold fluid are mixed together and well agitated, very ſoon an uniform is produced. This equality muſt proceed either from a communication of heat from the warmer to the colder fluid, agreeably to the common opinion, or from a perfect intermixture of hot and cold particles, according to the notion of Count Rumford. To which cauſe it ought to be attributed, I conceived I might diſcover, by aſcertaining whether, after ſuch an intermixture, any ſeparation of the hot and cold portions took place. If the equilibrium of temperature be owing to intermixture without interchange of caloric, the hotter particles, as ſoon as the agitation ceaſes, ought, by reaſon of their greater rarity, to accumulate, to a certain degree, at the ſurface, and there exhibit a temperature above the common one.

Other experiments of mixing fluids.

I firſt tried water, and mixing this fluid boiling-hot, with an equal quantity nearly ice-cold, in a ſtoppered glaſs jar, I ſhook them well for a ſhort time.

When hot and cold water are mixed, they acquire a common temperature throughout, and never ſeparate.

I then noticed the reſulting temperature, and raiſing the ball of the thermometer towards the ſurface, I had an opportunity of obſerving, that it never roſe the ſmalleſt portion of a degree above the common temperature which had been eſtabliſhed.

I next made a ſimilar experiment with alcohol, ſelecting it on account of its remarkable dilatability. I ſhook well, for half a minute, a mixture of equal parts of alcohol at tempe- 40° and at temperature 170°. The reſulting temperature of the maſs was 140°.

The ſame effect was found with alcohol.

Now, if this was a mixture of particles at 40° and at 170°, as the difference of ſpecific gravity between the fluid at theſe temperatures is very conſiderable, ſome ſeparation of the warmer

and lighter particles from the others, ought, I conceive, to have taken place. The temperature of the top, however, never indicated the arrival of warmer particles. It never aſcended above the point of equilibrium.

From theſe experiments I concluded, that the uniformity of temperature was eſtabliſhed by an actual communication and interchange of heat between the particles.

Count Rumford remarked that the mixture might be too complete to allow of ſeparation:

It may not, however, be improper to ſtate, that Count Rumford, with whom ſeveral years ago I had the pleaſure of converſing upon this ſubject, alledged, that the intermixture might be ſo complete as to prevent any ſeparation whatever.

If it be a property eſſential to fluidity, that heat cannot paſs from one particle to another, the particles of different fluids ought to be equally incapable of imparting caloric mutually to each other. Unfortunately, however, for the ſpeculation, the the caloric is ſo communicated Though, *à priori*, I entertained no doubts reſpecting the reſult of the experiment, I poured a quantity of olive oil which had been heated by immerſion in a veſſel of boiling water for half an hour, upon an equal volume of water of 38°, and agitated the mixture, by ſhaking for a quarter of a minute. The common temperature produced was 78°, and the heat had gone from the oil into the the water; for when the fluids ſeparated, and had arranged themſelves according to their ſpecific gravity, both of them had the ſame temperature of 78° *.

but oil and water do acquire the common temperature by mixture, and exhibit the ſame when ſeparate.

The experiments of the two deſcriptions now recorded, left on my mind little doubt that the Count had overſtrained his concluſions.

Exp. 4. The tall jar of Exp. 3, containing water at 40°, was cooled round its *lower part* by ice and ſalt.

† *Exp.* IV. I took the ſame tall jar, and ſtopping the tubulature with a cork, I filled it with water of temperature 40°, and placed it in a pan After ſuſpending two thermometers, as in experiment firſt and ſecond, I poured a mixture of ice and ſalt into the pan, to the depth of 4.2 inches, the air of the room being 40°, as in the laſt experiment.

The temperature fell as quickly at top as at bottom:

	Bottom.	Top.	Air.
Eleven o'clock,	40°	40°	40°
In 10 minutes,	38+	38+	

* This is alſo very ſtrikingly the caſe with mercury and water.—N.

† The text is here reſumed.

In

	Bottom.	Top.
In 20 minutes,	38°—	38°—
— 30 ———	37—	37—
— 40 ———	36	36
— 60 ———	35.5	35.5
— 80 ———	35	35
- 100 ———	34.5	35
- 120 ———	34—	34
— 8 hours,	34—	34

A cruſt of ice began to form on the inſide of the glaſs when the water in the axis of the bottom and of the top was at 36°. In the courſe of the experiment, it became at leaſt an inch thick.

We learn from this experiment, that cold applied to the lower part of a cylinder of water, nearly 18 inches long, and having the temperature of 40°, is actually as ſpeedily perceived at the ſummit as in the axis of that part, on the external ſurface of which it immediately acts. As fluids conduct heat ſo very tardily, this can only ariſe from currents of cooled water aſcending from the bottom, and theſe cold currents cannot move upwards, were not the water of them ſpecifically lighter than that of the incumbent warmer fluid.

whence it is concluded that the cooled water aſcended from the bottom from its greater rarity below 40°;

The water, therefore, which at the bottom is cooled by the contiguous frigorific mixture, muſt be expanded by the loſs of caloric.

This experiment ſecures full force to the laſt, as it obviates the objection already noticed, and alſo precludes another. I have already ſtated, that it may perhaps be alledged, that the fluid at the top, in experiment third, though cooled to 32° did not deſcend, becauſe below 40°, the contraction is ſo trifling, that it does not occaſion a difference of ſpecific gravity ſufficiently great to cauſe the particles to deſcend, when oppoſed by the inertia and tenacity of the fluid through which they have to fall: or it may be conceived, that the deſcent is ſo tardy, that time is given to the ambient air or ſubjacent fluid to furniſh heat enough to raiſe the temperature of the deſcending ſtream, and by that arreſt it in its downward courſe.

and the objections to Exp. 3. are removed.

But from the particulars above recorded, it is manifeſt, that the change of denſity between the temperature of 32° and 40° is quite ſufficient to put into motion the particles, and to

enable them to overcome the obſtacle ariſing from inertia and tenacity, and to withſtand the arreſting effects of atmoſpheric heat.

Though theſe experiments, and ſome others of a ſimilar nature, carried conviction to my mind, and perfectly ſatisfied me reſpecting the reality of the anomaly of water, I determined to vary ſomewhat the mode of making the experiment, ſo as to obtain ſtill more ſtriking reſults.

Another experiment with a ſtill taller jar, 21 inches high.

For the fifth experiment, I uſed an apparatus which conſiſted of a ſtill taller jar. It was 21 inches high, and 4 in diameter. I adjuſted at the middle of its height a perforated baſon of tinned iron, 2 inches in depth, and 10 in diameter. As this baſon embraced the middle of the jar, I could, by filling it with hot water, or a frigorific mixture, apply heat or cold to the middle portion of the fluid in the jar, and thence, by the thermometer, learn what courſe the heated or cooled fluid ſhould take.

Exp. 5. The laſt mentioned jar was filled with ice-cold water. Heat was applied to a zone of two inches near the middle by means of warm water in a circumambient veſſel.

Exp. V. I filled the jar with water at 32°. I placed it upon ſeveral folds of thick carpet, previouſly cooled to the ſame degree. The air of the room going from 33° to 35°, I introduced two thermometers, as in experiments firſt and ſecond. I then poured water of temperature 68° into the baſon, and by means of a ſpout ariſing from the ſide of it, and a ſyphon connected with a reſervoir of water at the temperature now mentioned, I renewed the contents of the baſon frequently, but without cauſing any agitation.

	Bottom.	Top.	Air.
At commencement,	32°	32°	33—35
In 10 minutes,	35	32	
— 15 ———	36—	32	
— 20 ———	36+	32	
— 25 ———	37	33	
— 30 ———	38	33	
— 38 ———	38+	33	
— 45 ———	39—	33	
— 50 ———	39+	44	
— 55 ———	39+	45	
— 60 ———	39+	48	

Thetemperature roſe below to 36° but remained unchanged at top: but after the lower water had attained 39° it became ſtationary, and the temperature at top ſoon roſe to 48.°

*** From this time I charged the baſon with water of temperature 88°, and renewed it frequently.

Nothing can be more deciſive with regard to the queſtion in diſpute, than the particulars of this experiment. Heat is applied

applied to the middle of a column of ice-cold water. The heated portion has an equal ſhare of the column of cold fluid above it and beneath it. There is nothing to determine its courſe in one direction or another, excepting its actual change of denſity.

The thermometer evinces that the warm current ſets downwards, and carries the increaſed temperature to the bottom. There, this inſtrument indicates the ſucceſſive riſe of ſeveral degrees, before the ſurface indicates the ſmalleſt acquiſition of heat.

Whence it is deduced, that a *warm* current of water between 32° and 39°+ deſcended becauſe *denſer* and that when the temperature was more than 39+ the *warm* current aſcended becauſe *rarer*.

The inference is plain, that the cold water is contracted by the heat.

The change of the effect of heat is equally well illuſtrated by this experiment.

No ſooner did the inferior portion attain the temperature of 39°, than the heated fluid altered its courſe, and, by aſcending, carried the increaſe of temperature very rapidly to the ſurface, ſo that it ſoon ſurpaſſed the bottom, and continued to riſe, while the other remained ſtationary.

Exp. VI. I filled the jar uſed in the laſt experiment with water of temperature 39½°, the air and the ſupport being at 39°. Diſpoſing the thermometers in the uſual manner, I introduced a mixture of ſnow and ſalt into the baſon.

Exp. 6. The ſame jar was filled with water at 39½° and a freezing mixture applied to the middle zone. The fluid at bottom was ſcarcely changed, but that at top froze.

	Bottom.	Top.	Air.
At commencement,	39.5	39.5	39°
In 10 minutes, -	39+	38+	
— 25 ———	39+	36.5	*** At this time
— 35 ———	39	36—	ice began to be
— 55 ———	39	35	formed on the
— an hour and 10 min,	39—	34+	ſide of the veſ-
——— 35 ——	39—	34—	ſel.
— 2 hours, -	39—	33+	

This experiment ſpeaks in as decided language as the preceding. It ſhows that when a portion, in the middle of a column of water at temperature 39.5 is cooled, the colder fluid riſes, and does not deſcend through the warmer maſs, and preſents the unequivocal demonſtration, that water of temperature 39½° is actually expanded by loſing heat.

So that the water cooled below 39½ did riſe by expanſion.

The different experiments which I have in detail recorded, agree perfectly with each other in the evidence they give relative

The general fact is, that heat cauſes ice-cold water to contract to 40°, and afterwards to expand.

relative to the ſubject of inquiry. The general import of them is, that water which is ice-cold, or a few degrees warmer, when heated, becomes ſpecifically heavier,—that water of 40° when heated becomes ſpecifically lighter,—that water above 40°, by the loſs of heat, or by cold, is rendered ſpecifically heavier; and that water below 40° is, by the ſame cauſe, rendered ſpecifically lighter.

Such being the general import, the concluſion is irreſiſtible, that heat, in low temperatures, cauſes water to contract, and at ſuperior temperatures to expand. The opinion, therefore, is founded in truth, that water poſſeſſes a peculiarity of conſtitution in relation to the effects of caloric, and that it is, within a ſhort range of temperature, an exception to the general law of "expanſion by heat."

The greateſt denſity lies between $39\frac{1}{2}°$ and 40°.

So far as I can judge from theſe experiments, I am diſpoſed to believe that the point at which the change in the conſtitution of this fluid in relation to heat takes place, lies between $39\frac{1}{2}°$, and the 40th degree.

I am not at preſent aware of any objection to the method I have followed in eſtabliſhing this ſingular anomaly, and in removing any doubts which may have ariſen from the unavoidable influence which the inſtrument muſt have in the mode of conducting the inveſtigation that had previouſly been adopted.

Theſe experiments ſhew the nature of the change, but not its amount. Whether the expanſions and contractions be the ſame at equal intervals from 40°?

The plan of operation above deſcribed, however, only aſcertains the fact; it gives no data for aſcertaining the amount of the anomalous effect of heat.

I have already ſtated, that M. de Luc alledged, that from the temperature of 41°, the expanſion occaſioned by cold was very nearly equal to that produced by the ſame number of degrees of heat; and conſequently that water poſſeſſes the ſame denſity at any given number of degrees of temperature above and below 41°. The firſt experiments of Mr. Dalton appeared to confirm this opinion, and to enlarge the range to which it applied, by extending it to temperatures as far below 32°, as water allows itſelf to be cooled before it begins to freeze. From one circumſtance that conſtantly occurred, I am inclined to think, that the amount of the dilatation by cold is inferior to that cauſed by heat.

During the heating or cooling of water below 40°, the difference of temperature between the top and bottom of the fluid was leſs than what occurred during the cooling or heating

ing of the fluid through the ſame number of degrees above it; and I conceive that, when other circumſtances, but particularly the rate of the change, are alike, the difference of temperature between the upper and lower parts of the fluid, as it depends upon, may prove a meaſure of the difference of denſity.

Alcohol, when heated or cooled, preſents, by reaſon of its greater expanſibility, a greater difference of temperature in theſe ſituations than water; and upon the ſame principle I infer, that water from 40° is more expanded by an equal number of degrees of elevation than of depreſſion.

It is a difficult problem to explain, how theſe contrary changes by heat are effected.

As the concurrence of the teſtimony of the experiments above related with the general opinion, will probably remove every doubt reſpecting the matter of fact, it remains a very difficult problem for thoſe who are fond of philoſophical inveſtigation, to explain how heat ſhall occaſion in the ſame fluid, without producing any alteration of mechanical form of chemical condition, at one time contraction and at another expanſion, and to reconcile the contractive effect to the conceived notions of the mechaniſm of the operations of this energetic agent.

The queſtion ſtated.

When heat cauſes expanſion, it is imagined to act by inducing a repulſion among the particles of bodies, which, oppoſing and overpowering the coheſive attraction, cauſes the particles to recede.

In what manner, then, the addition of heat can occaſion, or allow, the particles of water to approach each other, and how the ſubtraction of it can make them retire to a greater diſtance, I confeſs I can in no meaſure comprehend.

Sir Charles Blagden's explanation; viz.

An explanation, abundantly plauſible at firſt view, very readily ſuggeſts itſelf to every one who is aware of the great and forcible expanſion which happens to this fluid at the moment of its congelation. It it ſtated by Sir Charles Blagden, in the paper already quoted.

As water expands in freezing by virtue of a new arrangement of the particles,

The remarkable dilatation which water experiences at the inſtant of being converted into ice, is very generally aſcribed, and I preſume very properly, to a new arrangement which the particles aſſume, determined probably by their polarity; by which one ſide of the particle A is attractive of one ſide of B, while it is repulſive of another.

Now,

it is probable, that the arrangement and the expanſion may begin before ſolidity enſues,

Now, if this polarity operates with ſo much energy as to impart almoſt irreſiſtible expanſive force at temperature 32°, it is reaſonable to ſuppoſe that it may begin to exert its influence, though in a far inferior degree, at temperatures ſomewhat more elevated. The expanſion, therefore, that takes place, during the fall of temperature from 40°, may be imputed to the particles beginning or affecting to aſſume that new arrangement which their polarity aſſigns them, in which arrangement theſe particles occupy more ſpace than before.

and the contrary.

Again, when heat cauſes water of 32° to contract, upon the ſame principle, it may be conceived to operate, by counteracting the ſmall portion of the diſpoſition to polarity that ſurvives the liquefaction.

I am afraid that we cannot reſt ſatisfied with this explanation. We muſt not be deceived by the plauſibility of it.

The ſtate of perfect fluidity depends upon the circumſtance, that the particles of any body admit of ready motion upon each other, and that the change of relative ſituation meets with little or no ſenſible reſiſtance.

Objection. This advance towards congelation, ought to impair the fluidity:

Water certainly poſſeſſes fluidity in a great degree, and its particles muſt of courſe encounter but little reſiſtance, as they glide the one upon the other. But if theſe particles ſhall begin to exert any degree of polarity, by which certain faces become more diſpoſed to attach to each other than certain others, this tendency would neceſſarily oppoſe that indifference with regard to poſition, which is eſſential to fluidity, and of courſe muſt impair the fluidity, and induce ſome degree of tenacity or viſcidity.

which does not appear to be the caſe.

To appearance, however, water at 32° has its fluidity as perfect as at temperatures conſiderably elevated. Unwilling to truſt to appearance, where experiment might decide, I have attempted in various ways to aſcertain whether the water ſuffers any ſenſible diminution in this reſpect while it is expanded by cold. The following method I deem the moſt correct.

Experiments with Nicholſon's gravimeter.

For the purpoſe, I employed a gravimeter, the one contrived by Mr. Nicholſon for diſcovering the weight and ſpecific gravity of ſolids.

This is a convenient inſtrument, but, unfortunately, it is by no means ſo tickliſh as a balance. Duly loaded, ſo as to be equiponderant with the water in which it is plunged, Mr. Nicholſon

Nicholſon ſays, it is ſenſible to the 20th part of a grain*. The one I have, though its ſtem be ſlender, is ſcarcely ſenſible to leſs than two or three twentieths of a grain.

The want of ſenſibility in the gravimeter ariſes, in a great meaſure, though not entirely, from a certain degree of tenacity ſubſiſting among the particles of the fluid; and any thing that tends to increaſe this tenacity, muſt, in the ſame proportion, augment this want of ſenſibility.

To aſcertain whether any ſenſible change in the tenacity or fluidity accompanies the expanſion of water by cold, which the theory requires, I examined the mobility of the inſtrument when immerſed in water at different temperatures. I firſt plunged it into this fluid, heated to between 60° and 70°. Under due loading, which ſunk it to the mark on the ſtem, it was not ſenſible to a weight leſs than two or three twentieths of a grain.

It roſe and fell in warm, and

I then tried it in ice-cold water, and found that its ſenſibility was in no perceptible degree impaired. The coldneſs of the water, it muſt be remembered, cauſes ſome degree of contraction of the gravimeter. This contraction cannot fail to render the inſtrument in ſome ſmall meaſure more ſenſible, and, ſo far as it goes, to counteract the ſluggiſhneſs produced by any increaſed tenacity in the fluid.

in ice-cold water with equal facility.

But as the body of the inſtrument is made of glaſs, the amount of the contraction muſt be very ſmall, and the change of ſenſibility ariſing from it ſo very trifling, as certainly by no means to obſcure ſuch an effect as an increaſe of tenacity would occaſion. I therefore with ſome confidence conclude, that the fluidity of the water is not ſenſibly diminiſhed, and conſequently that the polarity has not begun to exert any ſenſible influence; it can ſcarcely, therefore, be accounted the cauſe of the dilatation.

Whence the author concludes that the fluidity is not ſenſibly changed.

* Perhaps the difference of ſenſibility in my inſtrument, and that of the learned Profeſſor, may have ariſen from a difference of the diameters of the ſtems. Mine was of one-fortieth of an inch. It was well rubbed with a clean linen cloth, which rendered the ſurface equally diſpoſed either to deſcend or aſcend; and the inſtrument was not judged to be in equilibrio with the fluid, except when the ſurface about the ſtem was neither prominent nor depreſſed. This was eaſily known by the reflected image of the window frame, or other objects being ſeen cloſe to the ſtem without diſtortion.—N.

Annotation.—W. N.

Sir Charles Blagden's theory of expanſion of water by heat does not ſuppoſe that it ſhould be leſs fluid.

It does not ſeem to me that Sir Charles Blagden's explanation does neceſſarily imply that the fluidity of the maſs taken as a whole, ſhould be ſenſibly impaired when tried by the application of a mechanical teſt. It might be impaired in the ſame manner as the water is affected by mixing ſmall floating fragments of a ſolid along with it. When a ſaline ſolution which would become ſolid by cold, ſuch for example as the ſulphate of ſoda, is cooled below its point of congelation, the cryſtals will be differently formed according to circumſtances.

Inſtance of cryſtallization differing according to circumſtances.

If the fluid be gently ſhaken or made to oſcillate, a ſhower of minute cryſtals will gradually fall through the fluid; and the whole maſs will be a conſiderable time before the cryſtallization is finiſhed; but if, inſtead of this method of agitation, the glaſs be ſcratched by a quill underneath the fluid, in Sir Charles Blagden's way, or if a ſmall inſtrument, having a cryſtal of the ſalt adhering to it, be dipped into the ſolution, the cryſtals will radiate with great rapidity from that centre of perturbation, and in a few ſeconds the whole of the ſolution will become rigid. This common and very ſtriking experiment of chemical lecturers, ſeems to me to indicate at leaſt a poſſibility that ſmall cryſtals of ice may be formed and float diſtinctly from each other in water, at 40 degrees and lower: and I think the metals afford us a number of inſtances in which a conſiderable interval of temperature is found to be between the commencement of cryſtallization and the ſolidification of the whole maſs. In pewterers ſolder the interval is not leſs than 40 degrees. This hypotheſis of ſuch diſſeminated particles of ice, which ſeems to be nothing more than an expreſſion of Sir Charles Blagden's theory in different words, will explain why the colder water ſhould be lighter;—namely, becauſe it muſt contain more ice, and alſo why the expanſion ought not to begin but at ſome definite temperature.

Whence it is conjectured that the expanſion of water by cold may ariſe from minute cryſtals of ice in the fluid.

A meaſure of the greater or leſs fluidity of bodies is very deſirable.

Though it does not appear to me that the theory of Sir Charles muſt neceſſarily imply a change in the mechanical reſiſtance of water from what may be called rigidity; yet there are many other reaſons why philoſophers ſhould be deſirous of meaſuring the variations of fluidity in bodies: that is to ſay, the greater or leſs facility with which their parts are moved amongſt each other. The ingenious attempt of Dr. Hope to aſcertain this from the reſiſtance made by a fluid to the perfo-

Dr. Hope's trial may be modified probably by a

ration

ration of its ſurface by a cylindrical ſolid, is liable to the objection that it ſuppoſes the attraction or repulſion between the ſolid and the fluid to remain unchanged by variations of temperature; whereas the contrary ſeems moſt probable. The doctor's experiment muſt be grounded upon a poſition that the greater the depreſſion or the greater the elevation of a fluid round a ſmall cylinder partly immerſed in it, the greater muſt be the reſiſtance from imperfect fluidity. But theſe effects are evidently as much governed by the attraction or repulſion of the ſolid with regard to the fluid as by the reſiſtance which the experiments are intended to meaſure. I have ſomewhere read that water clocks and other inſtruments for meaſuring time, by the paſſage of water through ſmall holes, go ſlower in cold weather. This may ariſe from contraction of the hole, though my author aſcribes it to imperfect fluidity. After ſome meditation on this problem it ſtill appears to me to be ſurrounded with difficulties. Perhaps it may be one of the beſt methods to ſuffer the fluid to drop from a capillary ſyphon in different temperatures. I am diſpoſed to think that the drops would be ſmalleſt and the whole quantity in a given time greateſt when the fluidity was the moſt perfect, or at leaſt when the adheſion of the particles of the fluid to each other was the leaſt. But even here the attraction of the ſmall capillary extremity of the tube from which the drop would fall would require to be conſidered; and on this account the method would be preferable (if ſo) to Dr. Hope's only becauſe the repetition of a great number of drops or quantity of effluent water would give a greater degree of preciſion to the reſult.

change in the attraction or repulſion between the water and the ſtem of the gravimeter.

Water clocks are ſaid to go ſlower in cold weather; becauſe the water is leſs fluid.

Suppoſition that moſt water would drop from a capillary tube when it was moſt fluid.

Is it likely that the rope pump turned regularly a certain number of turns in a given time would raiſe more water when coldeſt and leaſt fluid? If it did not might we not infer that the fluidity of water is not ſenſibly affected by change of temperature?

Will the rope pump ſhew a difference in the tenacity or fluidity of water hot or cold?

Obſervations

IX.

Obſervations on Turf, from the German Rathbeger ſur alle Stænd. *By* DOCTOR COLLENBUSCH.

IT is not very probable that a man placed beſide a fountain of pure water ſhould ſuffer himſelf to die of thirſt through neglect of uſing it, or poſſeſſing food in abundance, ſhould not appeaſe his hunger with it; nevertheleſs inſtances of this kind are not wanting.

Wood fuel very ſcarce in Germany. Other matter may be ſubſtituted for it.

Every one complains in Germany of the ſcarcity of wood for fuel. It is known that ſubſtances have been found in other places which can ſupply its place, and that they have been formerly uſed here; but all this cannot induce any one to ſearch for *turf*.

Ungrounded prejudices prevent the uſe of turf for fuel.

It is eaſily conceived that proprietors of woods, through the fear of having their profits diminiſhed, ſhould endeavour to perpetuate ancient prejudices, and to extend the opinion that the plague only ceaſed its ravages ſince the uſe of *turf* for fuel has been diſcontinued; but it is difficult to imagine that magiſtrates inſtead of encouraging the preparation of this fuel, ſhould endeavour to prevent thoſe from doing ſo, who wiſhed to engage in it.

Uſed in Germany from the moſt remote periods.

It is very likely that the diſcovery of the uſe of *turf* as a combuſtible was firſt due to chance; and beſides the uſe of this fuel in Germany has been continued from periods more remote than any written documents extend to.

Various erroneous opinions formed of its production and uſe.

The principal cauſes which have prevented the ſearch after *turf*, are the erroneous opinions which have been formed of the manner in which it has been produced, of its preparation, and its uſe; ſome of which are as follow.

1ſt That turf is found in veins like metals.

Some think, for example, that *turf* has been formed at the moment of the creation, ſuch as it is now found in the earth, and that there are veins of turf, as there are of iron, copper, tin, and other metals; but experience proves the falſity of this opinion, for there is found in almoſt all parts of Germany turf covered with more or leſs earth, (if only a proper ſearch be made for it) beneath which layers of trees may be ſeen, which proves that there formerly were foreſts in the ſame places.

Others

Others believe that at the time of the deluge vaſt foreſts were overthrown, and afterwards covered with herbs, reeds, and other plants, and that theſe vegetables having rotted by degrees, became at laſt this black combuſtible maſs reſembling earth, which muſt have required an enormous quantity of vegetables, as plains of many leagues ſquare are found covered with beds of it to the depth of more than 25 feet, beneath which trees are diſcovered of great hardneſs, and almoſt petrified.

2d That it was formed at the deluge.

Others imagine that it is more probable that the ſea tranſported the materials of the turf from the weſtern countries to the eaſtern, and covered with them the trees which are found buried beneath the turf. It is very true, that theſe trees have their roots turned towards the weſt, and their heads to the eaſt. But then it is difficult to explain how this ſubſtance could be carried to countries diſtant from the ſea, and even to the tops of the higheſt mountains in upper Saxony, on the *Brocken* and the *Alps*.

3d That it was tranſported by the ſea from the weſt.

Many perſons are of opinion that torrents and rivers have drawn together and depoſited leaves and branches of trees on the low grounds, and that they have thus accumulated the conſtituent elements of the turf; but this cannot take place in countries in which no large rivers are found, nor on high mountains. The microſcope clearly ſhews that turf, eſpecially that kind which is from the ſurface of the earth, is compoſed of moſſes, herbs, ruſhes, and other vegetables, and their roots ſtrongly interlaced, of which the greateſt part is changed into earth.

4th That it was waſhed down by torrents.

The microſcope ſhews it compoſed of vegetable fibres.

Paper has actually been compoſed from turf, and the water which has ſettled in turbaries is uſed to tan leather, which proves that it is principally compoſed of vegetables. Chemical reſearches have alſo diſcovered in it a mineral reſin which principally promotes its combuſtibility. It appertains then partly to the vegetable, and partly to the mineral kingdom.

Paper made from it and leather tanned by its water.

Turf may be produced artificially, by digging trenches 6 feet deep, and from 15 to 20 feet ſquare; the trenches become filled with water, and produce the firſt year a green ſlimy moſs, the ſecond year this moſſy vegetation covers the water to the height of two feet, and a great quantity of filaments are diſcovered in it, mixed with leaves and flowers, in the third year a ſtratum is eſtabliſhed, which attracts the duſt and the ſeeds which

Turf produced artificially by ſinking deep and wide trenches, which fill up by vegetation.

which float in the air, and engender a quantity of marſh plants, of reeds, and of herbs, which the fourth year become ſo heavy that they fall to the bottom. They then become compreſſed there, and by ſucceſſive repetitions of this operation, all the trench becomes filled up in the courſe of 30 years; however this turf would probably require 100 years before it would equal the ancient turf.

Three ſpecies of it.

Although this turf is always the ſame in its conſtituent parts, it nevertheleſs differs in having theſe parts variouſly mixed, which occaſions its being divided into three ſpecies. The

1ſt The ſurface turf.

firſt comprehends the *ſurface turf*, and is the moſt common kind; it is found almoſt every where; but it contains in ſome places more combuſtible matter, which makes its colour vary.

Found wherever water ſtagnates and is covered with weeds,

This ſpecies is always ſure to be found wherever places are diſcovered where the water ſtagnates, whether on plains, elevations, or declivities, in ſuch a manner as to form a thick blueiſh cruſt, and depoſits a yellow mud; or where the ſoil is covered with moſs, reeds, ruſhes, or ridges; and if at the ſame time the feet of the paſſenger ſinks into the looſe ſoil, if the earth bends beneath his feet; if trees are perceived (which are commonly little pines or fir trees, or ſometimes other kinds of trees,) covered with much moſs, inclined to one ſide, and half rooted up at the other by the wind; in all theſe places turf will be found near the ſurface, and it is only neceſſary to remove the ſod to perceive it. But this operation may be performed more quickly and eaſily with the Engliſh borer, which alſo will ſhew the depth of the bed.

and where trees covered with moſs are half up-rooted by the wind.

To procure it the water ſhould be drained off.

To procure the turf, the water ſhould be drained off, which is eaſy to do if the country is elevated or has valleys in its vicinity; but the operation is more difficult when the earth is level. As perſons are not always to be found capable of taking the levels of ground, the places ſhould be remarked where the water ſettled in ſpring when the ſnow melts; theſe places ſhould be marked by ſtakes, and afterwards the trenches ſhould be made to paſs this way which are to be dug, to let the water run off.

Eaſy method of finding the deſcent for the drains.

Method of preparing the ſurface turf.

To cut the turf an iron ſpade is uſed, which ſhould be neither round nor pointed, but terminating in a ſtraight line; this ſhould be ſcrewed down as far forwards as poſſible, along the ſide of a ſtretched cord, by a line 14 inches long and ſix broad; the detached part is ſeparated from the depth of three

inches

inches, at two ſtrokes of the ſpade, to the length of 16 inches, and 4½ inches broad, and this piece of turf is afterward divided in two.

In order that the pieces of turf may dry quickly, they ſhould be placed on planks, and diſpoſed ſo that the air might freely circulate between them, and that they could receive the rays of the ſun. Method of drying it.

When the turf is thus dried to a certain degree, it is placed under ſheds to compleat the drying; for if it was expoſed to the ſun till it was entirely dry, it would loſe its ſtrength and burn like ſtraw. Should not be dried too much in the ſun,

It is alſo diſadvantageous to cut a large proviſion of it for many years, for the laſt made is always the beſt. The upper and lower beds are alſo obſerved to be of inferior quality to thoſe in the midſt; the beſt turf is that of a brown colour inclining to black, is heavy, and its texture is traverſed by a ſmall quantity of roots; this kind produces a ſtrong and laſting fire, and its ſmell is very ſupportable. The more it is of a bright brown colour, the greater number of roots in it, and the lighter it is, the worſe is its quality. This ſort conſumes more ſpeedily, and may ſerve to advantage where a quick fire is wanted; its odour, it is true, is very diſagreeable, but its aſhes are excellent. or kept too long. Upper and lower beds of it the worſt. Beſt kind dark brown and heavy. Bright brown or reddiſh worſe.

The turf which inclines to a grey or yellow colour, and which is mixed with reed, is always the worſt ſort, but always good enough to heat kilns or ovens, and its aſhes are good; this ſpecies is ſeldom found below the depth of two ells; it is reproduced after ſeveral years. Grey or yellow ſort the worſt.

The ſecond ſpecies of turf is the *crumbling turf (moder-torf)*, this kind is found more abundantly in Holland; its cutting and preparation require much more pains than the ſurface turf. Second ſpecies, the crumbling turf.

The third ſpecies, or the *mountain turf*, is dug up from pits and galleries, and is reduced to regular forms like the preceding kind. Third ſpecies, the mountain turf.

It is objected to the uſe of turf that it cannot be employed as a ſubſtitute for wood in all the places where wood is burned; but it ſhould not be forgotten that wood itſelf is not fit for every work where fire is required; that in order to be employed in founderies it muſt be charred with much trouble, and with a loſs of two thirds of its weight, and that wood as well as turf, is of different qualities and produces different effects. Wood not proper for fuel in all caſes more than turf.

Turf

Turf may be charred.

Turf is alſo ſuſceptible of amelioration, eſpecially the ſurface turf, the crumbling turf, and the mountain turf likewiſe; for it may be reduced to charcoal, and will thus ſerve for every work which requires fire; and in this caſe it yields neither ſmell nor ſmoke. The more ſtrongly the turf is compreſſed before its carbonization, the more excellent is the charcoal.

Advantages from the uſe of turf.

To compenſate for the inferiority of turf to wood, granting that it is inferior, its uſe will prevent the great price which will otherwiſe neceſſarily be paid hereafter for timber for building, and will admit of the woodlands being proportionally reduced; the places alſo where the ſurface turf has been dug up, if it has not been from too great a depth, may ſerve for ſituations wherein to plant cabbages, beets, and madder, or they will ſerve for fiſh ponds. The uſe of turf will admit of the multiplication of manufactories which uſe fire, of mines and forges; aged perſons and children may be employed in preparing it; its aſhes form a good manure, and the mould which falls from it may eaſily be converted into aſhes.

Cabbages, &c. may be planted where it has been extracted.

Its uſe admits of having more manufactories where fire is uſed.

X.

Experiments on the remarkable Effects which take place in the Gaſes, by Change in their Habitudes, or elective Attractions, when mechanically compreſſed. *By* THOMAS NORTHMORE, *Eſq.* *In a Letter from the Author.*

To Mr. NICHOLSON.

Devonſhire Street, Portland Place, Dec. 17, 1805.

SIR,

The author's reaſon for early publication.

IT was my intention to have poſtponed troubling you with the following experiments upon the condenſation of the gaſes, until I had brought them to a greater degree of perfection; but being informed that ſeveral of them have already, by means of which I am ignorant, and probably in a mutilated ſtate, found their way to the preſs, any further delay ſeems improper. If then you deem the preſent communication worthy a place in your intereſting Journal, it is entirely at your ſervice.

His ſuſpicion that the affinities of the gaſes would be changed by compreſſion.

It had long ago occurred to me, that the various affinities which take place among the gaſes under the common preſſure of the atmoſphere, would undergo conſiderable alteration by the influence of condenſation; and the ſucceſs attending the *violent* method adopted by the French chemiſts, which violence did not appear to me requiſite, afforded additional encouragement to my undertaking ſome experiments upon the ſubject.

I communicated this to the late chemical operator in the Royal Inſtitution, a gentleman eminently converſant in the ſcience, and with whom I was then engaged in a ſeries of experiments: he not only approved of my deſign, but ſeemed to think it not improbable that an extenſive field might thus be opened to future diſcoveries. Whether theſe opinions are juſtly founded, is now left for you, Sir, and the public to judge.

Difficulties of the undertaking as to the inſtruments.

In entering upon a field entirely new, obſtacles were of courſe to be expected: nor without reaſon; for though I had applied to one of the moſt eminent philoſophical inſtrument-makers in London, Mr. Cuthbertſon, yet I began to fear, even at the outſet, that his ſkill would be ſet at defiance. The firſt inſtruments which he made for the preſent purpoſe were, a braſs condenſing-pump, with a lateral ſpring for the admiſſion of the gas by means of ſtop-cock and bladder; two pear-ſhaped receivers, one of metal of the capacity of ſeven cubic inches, and another of glaſs of about three and a half: theſe were connected by a braſs ſtop-cock, having a ſcrew at each end. The metallic receiver was ſoon found to be of little or no utility, as well on account of its liability to be acted upon by the generated acids; its being too capacious, and thus conſuming too large a quantity of gas: as becauſe, though the reſult of an experiment might thus be known, yet the changes which the ſubjects might undergo would neceſſarily eſcape obſervation. The glaſs receiver obviated all theſe difficulties, and one or two imperfect experiments were performed with it; but the ſtop-cock ſpeedily failed in its effect. For the power of the compreſſed gaſes was ſo great, partly from their elaſticity, and partly (where affinities had operated) from their corroſive quality, as abſolutely to wear a channel in the metal of which the plug was made, and thus

Condenſing-pump.

Receivers.

Various objections.

to effect their escape. But not to trouble you any further with the obstacles that occurred, and which are mentioned only to prevent unnecessary expence to others, I have at last, by Mr. Cuthbertson's assistance, procured a connecting-tube, to which a spring-valve is adapted that has hitherto answered every purpose. *See Plate XIV. Fig.* 2, 3, 4, 5, 6.

Instruments now used by the author.

Pump.

The instruments which I now use, are, 1st. An exhausting syringe; 2d. A condensing-pump, with two lateral springs for different gases; 3d. The connecting spring-valve; and lastly, glass receivers, which should have been of various sizes, but the one mentioned above having burst, that which I have

Glass receiver.

principally used in the following experiments, is of about five cubic inches and a quarter in capacity, and made of glass well annealed and a quarter of an inch in thickness. Besides these instruments, I have occasionally applied Mr. Cuthbertson's

Syphon gage.

double syphon-gage *(See Fig.* 6), by which the number of atmospheres condensed in the receiver, or rather the elastic power of the gases, may be measured; but this is rendered of less service, because a stop-cock must then be placed between the receiver and spring-valve, which frequently impairs the whole experiment; and also because, after a certain degree of condensation, and more particularly upon the admixture of the gases, new affinities usually take place, which tend

Eighteen atmospheres compression.

to diminish the elasticity: the greatest number of atmospheres my gage has yet measured, is eighteen. These, Sir, with some bladders and stop-cocks, various iron screw-keys, and a wooden guard for the legs in case of bursting, constitute the principal part of the requisite apparatus.

I now proceed to the experiments, premising that the first four were made with the imperfect apparatus, when the gas was continually making its escape through the stop-cock.

Experiment I.

Exp. 1. Hidrogen, oxigen, and nitrogen gave water, and probably nitrous acid.

Into the glass receiver, of three cubic inches and a half capacity, were compressed in the following order: Hidrogen, two (wine) pints; oxigen, two pints; nitrogen, two pints.* The result was, water which bedewed the inside of the receiver; white floating vapours (probably the gaseous oxide

* These gases therefore occupied about five times the capacity they were condensed into.—N.

of

of nitrogen); and an acid which reddened litmus paper. Mr. Accum was preſent at this experiment, and from his opinion, as well as from ſucceeding experiments, I have reaſon to think that this acid is the nitric.

Experiment II.

Exp. 2. The ſame, but the oxigen firſt.

As a difference of arrangement in the order of the gaſes tends conſiderably to vary the reſult, I repeated the former experiment (having firſt poured a little lime-water into the receiver) by injecting firſt the oxigen, about three pints, then equal quantities of hidrogen and nitrogen. Much of this gas eſcaped, owing to the imperfection of the inſtrument; but upon the affuſion of the nitrogen, the white vapours again appeared in the receiver; water ſeemed likewiſe to be formed; and ſome yellow particles were ſeen floating upon the lime-water. Theſe particles probably aroſe from the reſinous ſubſtance, uſed in faſtening on the cap of the receiver, being diſſolved by the nitrous gas formed during condenſation.

I would juſt obſerve, that the magnet *ſeemed* to be affected during this experiment; but as there is iron uſed in the machine, this may be otherwiſe accounted for.

Experiment III.

Exp. 3. Carbonic gas and hidrogen. Water and a changed gas.

Two pints of carbonic acid, and two of hidrogen, were ſubjected to condenſation. The reſult was, a watery vapour, and a gas of rather offenſive ſmell.

Experiment IV.

Exp. 4. Phoſphorus in condenſed air.

Trying to inflame phoſphorus by the condenſation of atmoſpheric air, the bottom of the machine (where it had been repaired) burſt out with an exploſion. This happened when I had immerſed the apparatus in water to diſcover where the air eſcaped. The receiver was full of the fumes of the phoſphorus, which was itſelf diſperſed in the veſſel of water. I afterwards repeated this experiment with the more perfect apparatus, but I could not inflame the phoſphorus, and the fumes which aroſe at firſt ſoon diſappeared. There was juſt enough acid (probably phoſphoric) formed on the inſide of the receiver to tinge litmus.

Experiment V.

Exp. 5. Repetition of Exp. 1. with better apparatus.

Having now the ſpring-valve, and new receiver of five cubic inches and a half capacity *, I poured in two ſcruples of ſolution of potaſh, and then injected two pints of hidrogen, two of nitrogen, and three of oxigen. This quantity was hardly ſufficient for the capacity of the receiver, and the reſult was only a ſmell of the gaſeous oxide of nitrogen, a few yellowiſh fumes, and ſcarce enough acidity to tinge the edge of the teſt paper: of courſe, I could not effect the formation of nitrate of potaſh.

Experiment VI.

Exp. 6. Nitrogen (firſt) and then hidrogen and oxigen.

I now determined to begin with the nitrogen, which always appeared to me to undergo the moſt important chemical changes, and therefore injected two pints of nitrogen, three of oxigen, and two of hidrogen. Upon the condenſation of the nitrogen, it ſpeedily aſſumed an orange-red colour, which upon the acceſſion of the oxigen, gradually diminiſhed, and at length diſappeared, though at firſt it ſeemed rather deeper. A moiſt vapour, coating the inſide of the receiver, aroſe upon the compreſſion of the hydrogen, which moiſture was ſtrongly acid to the taſte, coloured litmus, and, when very much diluted with water, acted upon ſilver.

Experiment. VII.

Exp. 7. The ſame, but different arrangement.

Nearly the ſame as the laſt, but with different arrangement. The nitrogen, three pints and a half, was firſt introduced; then the oxigen, two pints; and next the oxigen, three and a half. The nitrogen formed the orange-red colour as before; the hidrogen produced white clouds at firſt (*quære* ammonia?) which afterwards diſappeared, and the orange-red colour became lighter; but upon the affuſion of the oxigen, the colour did not diſappear as in the laſt experiment, but, if any thing, became darker. I then injected two pints more of hidrogen, but this had little or no effect upon the colour. Some vapour was generated, which was, as uſual, ſtrongly acid.

Experiment VIII.

Exp. 8. Nitrogen over lime-water.

Previous to the burſting of the ſmall receiver, I had put in it a ſcruple of lime, and condenſed upon it three pints o

* One fifth part of a pint very nearly.—N.

nitrogen.

nitrogen. The result was, a little reddish colour at first, which soon vanished. Upon repeating this experiment in the large receiver, I could produce no colour at all. In my present state of knowledge I am unable to account for this circumstance; but as soon as I get my new receivers of a smaller capacity, I mean to repeat the experiment.

Besides the above, I have made various other experiments with different gases, but I think it right to repeat them with greater accuracy before I submit them to the eye of the public: if upon that repetition they appear to me to be attended with results of sufficient importance to occupy a place in your Journal, I will take the liberty of communicating them to you, and am, Sir,

Your most obedient servant,

THO. NORTHMORE.

P. S. I think it necessary to add, that during the course of the above-mentioned experiments, there was a great variation of temperature in the atmosphere, from the heat of 70 degrees of Fahrenheit to the cold of 33.

Explanation of the Figures, by Mr. Cuthbertson.

Description of a valve for condensing.

Fig. 2, 3, 4, 5. Plate XIV. represents sections of the several parts of the spring-valve for the condensing syringe; *a* is a female screw, intended to receive the male at the end of the syringe; *b* is a square, to which is a key to screw it perfectly tight to the syringe: *d*, Fig. 3. is a female screw fitted to the male *c*; *c* is a male screw fitted to the female of the glass receiver; Fig. 4. is a round steel arbor, turned with a conical part and flat shoulder at *a*; *a b* is a spiral spring; Fig. 5. is a hollow brass cylinder serving as a cover and guide to Fig. 4; the piece Fig. 2. has a small hole drilled through the center, and turned out at the end *c*, so as to fill the cone *a*, Fig. 4; Fig. 3. is turned out at *f* so wide as to receive Fig. 5.

If the plane shank of 4 be put into the hole *a c* till its cone shut close into the hollow cone at *c*, Fig. 2. and the other, end with the spiral, covered by Fig 5. screwed tight upon the flat end of *c*, and *d* be screwed to *c*, all the joints being properly supplied with oil, and leathers, it is fitted for use.

Fig. 6. Represents a section of the condensing or double syphon gage, being a glass tube bent into the form of the figures,

figures, the end *a* is mounted with a braſs ſcrew, having a hole through it correſponding with the inſide of the tube, the leg *b c* is filled with mercury, and *d* is hermetrically ſealed: *d c* is divided into atmoſpheres.

XI.

Account of a Graphometer for meaſuring the Angles of Cryſtals In a Letter from Mr. Robert Bancks, *No.* 411, *Strand.*

To Mr. NICHOLSON.

SIR,

Great advantage of diſtinguiſhing minerals by their figure.

I NEED not point out to you, and to the learned readers of your Journal, how great the advantage will be, whenever the ſame may be realized, of diſtinguiſhing ſubjects of the mineral kingdom by their external appearance. This has long been done, with conſiderable preciſion, by operative men who have acquired their ſkill from continued practice, but without being able to communicate the knowledge they poſſeſs by any ſimple indications, ſuch as might be given in writing, or through the medium of the preſs. Neither need I on this occaſion point out how much we are indebted to the labours of Bergman, Romé dé l'Iſle, and above all, Haüy, for ſcientific inveſtigations of the forms of cryſtals, which at preſent bid fair to afford us criterions of the moſt extenſive uſe. My preſent object is to communicate what I hope will be thought one ſtep, however ſmall, towards facilitating the admeaſurement of their angles. In your firſt vol. at page 132, you have given an account of the graphometer of Carangeau, which is now conſiderably known and eſteemed. I have rendered that inſtrument ſomewhat cheaper, and eaſier in the execution, and more correct in its uſe. For the ſake of thoſe who may not have that volume at hand, I ſhall briefly ſtate, that the inſtrument conſiſts of a ſemi-circle, like that which I am about to deſcribe, and a pair of compaſſes or legs having their centre in the centre of the ſemi-circle, but capable of having their points drawn back, ſo as to admit of their application to any ſmall cryſtals. The arc of the ſemi-circle is

Cryſtallography.

Carangeau's graphometer for cryſtals.

divided

divided into two quadrants by an hinge, ſo that one part may be turned back out of the way of any mineral, which may require to be brought up towards the centre for admeaſurement; and the ſame arc can afterwards be reſtored to its place, in order to ſhew the degree and fraction of the angle.

In my improved inſtrument I avoid this joint, and obtain a much firmer framing by making my arc in the form of a protractor, as in Fig. 1. Plate XV. having an hollow centre at A, and a ſtud at B, both lying in the direction of that diameter which terminates the graduations. The compaſſes, or radii, or legs, are ſhewn in Fig. 2. ſeparate from the arc. Their centre C is made like thoſe of the common proportional compaſſes, and admits of the legs C D, C F being conſiderably lengthened or ſhortened when the two pieces are applied to each other. D E the fixed leg is repreſented as beneath F G the moveable leg or radius, and the lower end of the centre pin is made to fit the hole A preciſely, at the ſame time that the ſtud at B being admitted into the long perforation towards E, the piece D E becomes ſteadily attached to the ſemi-circle, as is ſeen in Fig. 3.

Improved graphometer. The ſemi-circle is entire, and the compaſſes or callipers are uſed ſeparately for meaſuring the cryſtal, and applied to the ſemicircle for reading off.

The uſe is obvious. The cryſtal muſt be meaſured by the detached compaſſes as in Fig. 2, which are much more handy for all deſcriptions of minerals than Carangeau's entire inſtrument; and when thus ſet, if fig. 2 be applied to fig 1, as before directed, the angle will be read of at the fiducial edge of G.

I hope you and your readers will conſider this as the uſeful ſimplification of a valuable inſtrument, and ſhall be happy to receive your ſanction by its appearing in a work ſo generally known and eſteemed as your Journal.

I am, Sir,

Your obedient Servant,

ROBERT BANCKS.

Nov. 1, 1805.

Accounts

ACCOUNT OF NEW BOOKS, &c.

Philoſophical Tranſactions of the Royal Society of London for 1805. Part II. Quarto 353 pages, with an Index, and Six Plates. Nicoll.

Philoſophical Tranſactions of the Royal Society.

THIS part contains the following communications, 1. Abſtract of Obſervations on a Diurnal Variation of the Barometer between the Tropics. By J. Horſburgh, Eſq. 2. Concerning the Difference in the Magnetic Needle, on board the Inveſtigator, ariſing from an Alteration in the Direction of the Ship's Head. By Matthew Flinders, Eſq. Commander of his Majeſty's Ship, Inveſtigator. 3. The Phyſiology of the Stapes, one of the Bones of the Organ of Hearing; deduced from a comparative View of its Structure, and Uſes in different Animals. By Anthony Carliſle, Eſq. F. R. S. 4. On an Artificial Subſtance which poſſeſſes the principal Characteriſtic Properties of Tannin. By Charles Hatchett, Eſq. F. R. S. 5. The Caſe of a full grown Woman in whom the Ovaria were deficient. By Mr. Charles Pears, F. L. S. 6. A Deſcription of Mal-formation in the Heart of an Infant. By Mr. Hugh Chudleigh Standart. 7. On a Method of analyzing Stones containing fixed Alkali, by Means of the Boracic Acid. By Humphry Davy, Eſq. F. R. S. 8. On the Direction and Velocity of the Motion of the Sun and Solar Syſtem. By William Herſchel, L. L. D. F. R. S. 9. On the reproduction of Buds. By Thomas Andrew Knight, Eſq. F. R. S. 10. Some Account of Two Mummies of the Egyptian Ibis, one of which was in a remarkable perfect State. By John Pearſon, Eſq. F. R. S. 11. Obſervations on the ſingular Figure of the Planet Saturn. By William Herſchel, L. L. D. F. R. S. 12. On the Magnetic Attraction of Oxides of Iron. By Timothy Lane, Eſq. F. R. S. 13. Additional Experiments and Remarks on an Artificial Subſtance, which poſſeſſes the principal Characteriſtic Properties of Tannin. By Charles Hatchett, Eſq. F. R. S. 14. On the Diſcovery of Palladium, with Obſervations on other Subſtances found with Platina. By William Hyde Wollaſton, M. D. F. R, S. 15. Experiments on a Mineral Subſtance, formerly ſuppoſed to be Zeolite, with ſome Remarks on Two Species of Uran-glimmer. By the Rev. William Gregor.

Tranſactions of the Royal Society of Edinburgh (being the Continuation of Part II. together with Part III. of the Fifth Volume) Edinburgh Quarto 100 *pages Continuation of Part II, and* 126 *Pages, Part III. No Plates.*

Tranſactions of the Royal Society of Edinburgh.

THE heads of memoirs and communications made to the Society ſince their laſt publication are diſquiſitions on the origin and radical ſenſe of the Greek prepoſitions, by Mr. James Bonar, and experiments on the contraction of water by heat, by Dr. Thomas Charles Hope. Theſe two papers of which the latter is inſerted in our Supplement, complete the ſecond part: and the third part contains the hiſtory of the Society conſiſting of the following articles. 1. Of the Diurnal Variations of the Barometer, by Mr. Playfair. 2. Aurora Borealis obſerved in Day-Light, by the Rev. D. Patrick Graham. 3. Phenomenon of Two Rain-Bows interſecting one another, by Mr. Playfair. 4. On the Combuſtion of the Diamond, by Sir George Mackenzie, Bart. 5. Remarks on the Baſalts of the Coaſt of Antrim, by the Rev. Dr. Richardſon. 6. Rule for reducing a Square Root by a continued Fraction, by James Ivory, Eſq. 7. Singular Variety of Hernia, by Mr. Ruſſel. 8. Concerning the Chartreuſe of Perth, by the Abbé Mann. 9. Explanation of the Old Word Skull or Skoll, by the Rev. Dr. Jamieſon. 10. Biographical Account of the late Dr. James Hutton, by Mr. Playfair. 11. Minutes of the Life and Character of Dr. Joſeph Black, by Dr. Ferguſon. 12. Appendix Liſt of Members elected ſince the Publication of the laſt Volume. 13. Liſt of Donations.

Academical Inſtitutions in America.

THREE Inſtitutions for the promotion of Natural Philoſophy and the Arts, having been eſtabliſhed in the united ſtates of America, not many months ago, of which no notice has hitherto appeared in this work, it is hoped the following account of them will not be unacceptable.

The firſt is an Academy of the Fine Arts, of which the firſt idea is due to Mr. Livingſton: The public were ſo ſenſible of its importance, that long before the arrival of the

plaiſter of Paris caſts, which he preſented to the infant Society, the number of ſubſcribers, at 25 piaſters each, amounted to 180.

The ſecond Inſtitution is a Botanic Garden in the neighbourhood of New-York; as yet but a ſmall part of the treaſures of the vegetable kingdom are to be ſeen in it, but the admirers of botany haſten to ſend to it every intereſting plant which is to be found in their vicinity. The charter of incorporation of the ſubſcribers, is entirely conformable to the views of the founders of this garden of plants, and according to cuſtom, enſures the permanency of the eſtabliſhment: when the hot houſes are finiſhed, it is expected, that the collection of every thing rare and moſt intereſting, produced by the ſouthern ſtates, will be compleated.

The third Inſtitution is an agricultural ſociety, eſtabliſhed at Waſhington, under the ſpecial protection of government. The preſident of the United States, who is a moſt enlightened agriculturiſt, the chief men of the adminiſtration, the ſenators, and the deputies of congreſs, are all members of it officially. The ſociety being now wealthy from the ſums granted by government, and the numerous ſubſcriptions of aſſociates and correſpondents, have purchaſed an handſome houſe, and a farm of thirty acres; they have alſo began a library; and are in poſſeſſion of the fine collection of ploughs, and other inſtruments of Agriculture, which formerly belonged to general Waſhington: the form of its adminiſtration, the number and the ſucceſſion of its members, the capital which it may poſſeſs (ſpecified in buſhels of corn) and its whole organization is regulated by its charter of incorporation; which conſtitutes this aſſociation a body politic, and fixes the perpetuity of its continuation: It is reported, that the anſwers which it returned to the numerous queſtions propoſed by the ſocieties of the different ſtates ſoon after its eſtabliſhment, will form a very intereſting work which will ſoon be publiſhed.

Criterion of Life.

Criterion of Life.

DR. STRUVE has contrived an apparatus which is mentioned in the foreign Journals, but not deſcribed. The object of its application, is to ſhew by means of galvaniſm, whether the appearance of death be real; a purpoſe ſufficiently intereſting to every human being, who has for a moment reflected on the ſatisfaction which recovery from apparent death muſt give to the friends and relatives of the individual ſuppoſed to be dead; and on the ſtill more impreſſive and dreadful incident of recovery after burial. Our galvanic and anatomical philoſophers will find no difficulty in applying this powerful agent to ſo good a purpoſe, in which the learned Doctor has the merit of taking the lead.

Mr. SESSKEN who has ſucceſsfully laboured in the conſtruction of a reflecting teleſcope of thirteen feet focus, has lately ſupplied the Obſervatory at Lilienthal, with two mirrors of fifteen feet focus and eleven inches aperture, which prove to be excellent, and bear the magnifying power of 2000 very well, on the proper objects, and at ſuch ſeaſons as are fit for making obſervations of this nature.

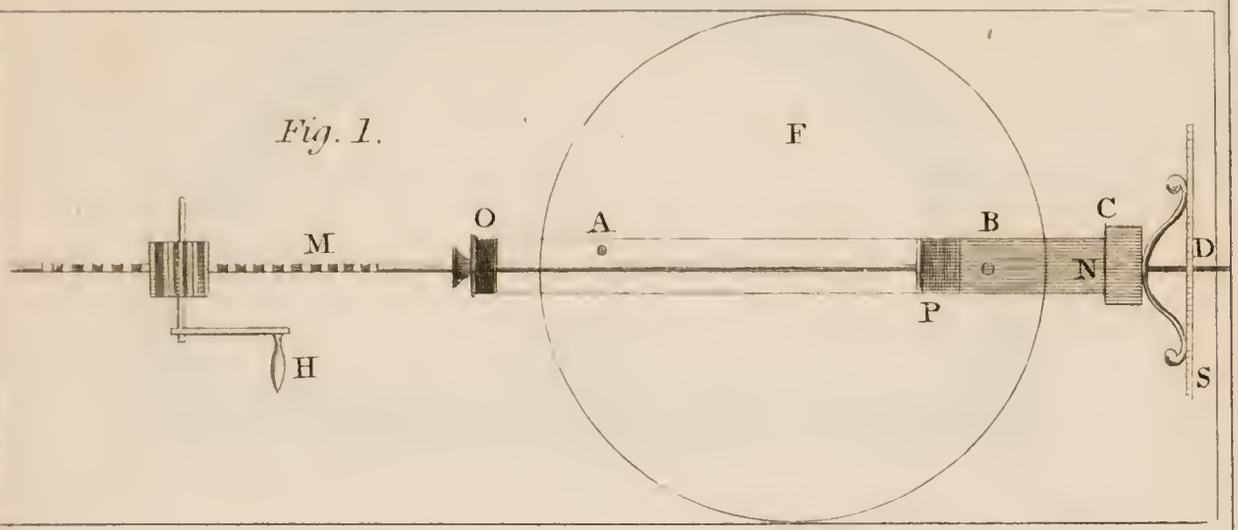
Numbering of Houſes.

Numbering of houſes.

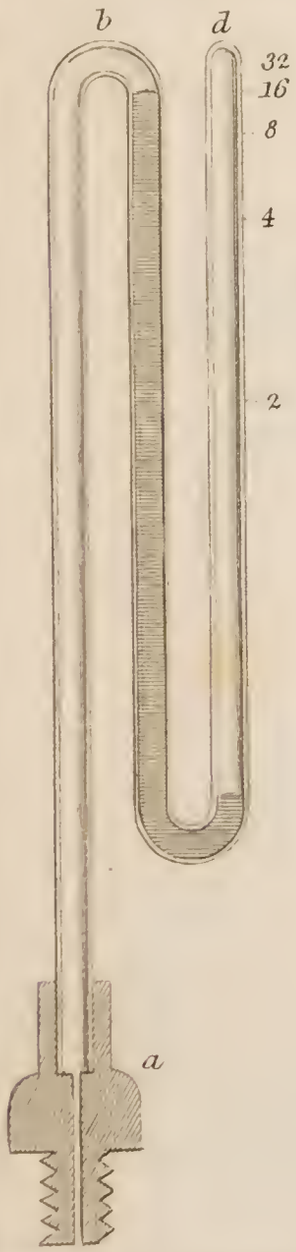
A NEW mode of numbering houſes has lately been adopted at Paris, which is attended with much advantage, and deſerves to be followed in this country. Over each door the numbers are painted in large diſtinct characters, and in conſpicuous colours; they are generally either brown or red, on a yellow ground, ſurrounded by a blue ſquare; but the principal ſingularity, which is the object of this notice, is, that all the odd numbers are placed at one ſide of the ſtreet, and the even numbers at the other; by which means, may be ſeen at once on entering the ſtreet, at which ſide of the way the houſe is, which is ſought for; by which much time may be ſaved, not only by its making it unneceſſary ever to croſs the ſtreet more than once, but alſo by its always preventing

preventing the trouble of returning back again on the other ſide of the ſtreet from that already paſſed, to find a particular number, which often happens, where the old method of numbering is uſed, from the order of the numbers proceeding regularly down one ſide of the ſtreet, and back again in the reverſed direction on the other; and which, when the ſtreets are very long, as many are in this metropolis, is often attended with ſerious inconvenience; but in the new method of numbering, this can never occur, as in it the numbers proceed in the order of progreſſion in the ſame direction at both ſides of the ſtreet.

Mr. Wright's new Air Pump.



Mr. Cuthbertson's Valve for a Condenser &c.



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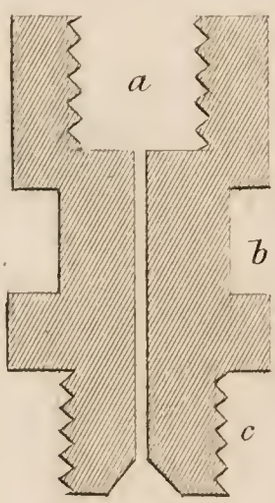

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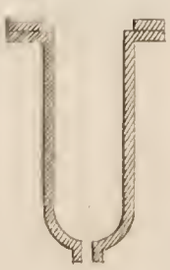

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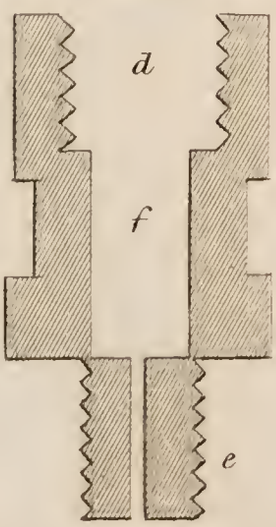
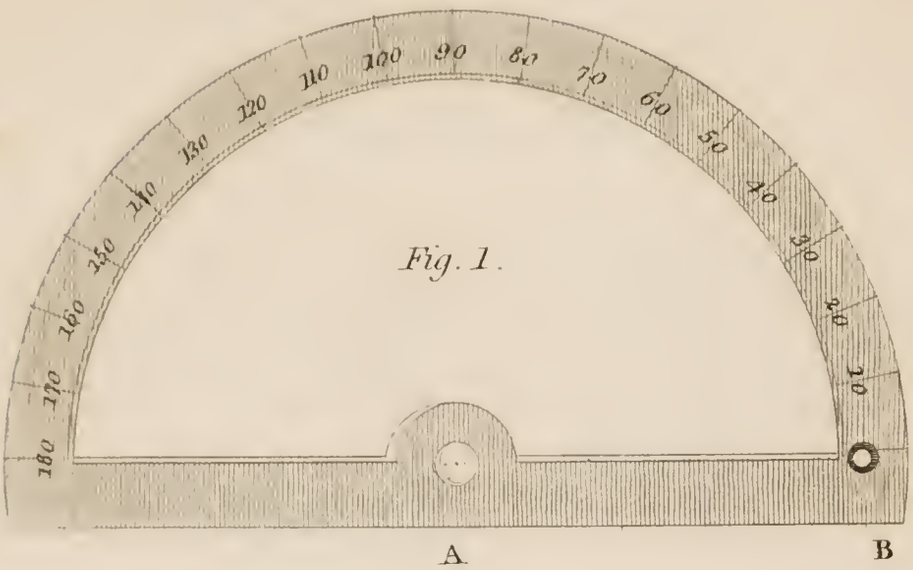


Improved Graphometer by Mr. Banks.



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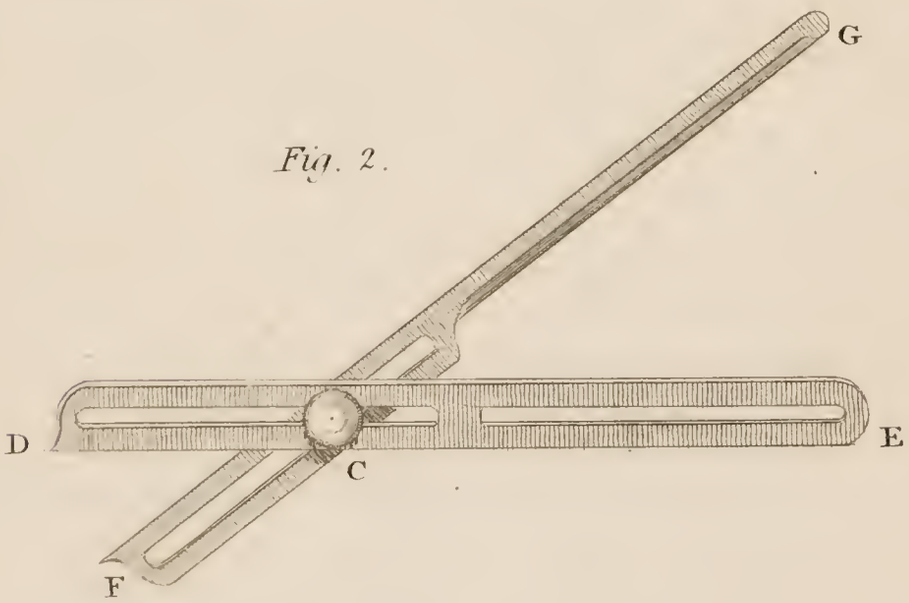

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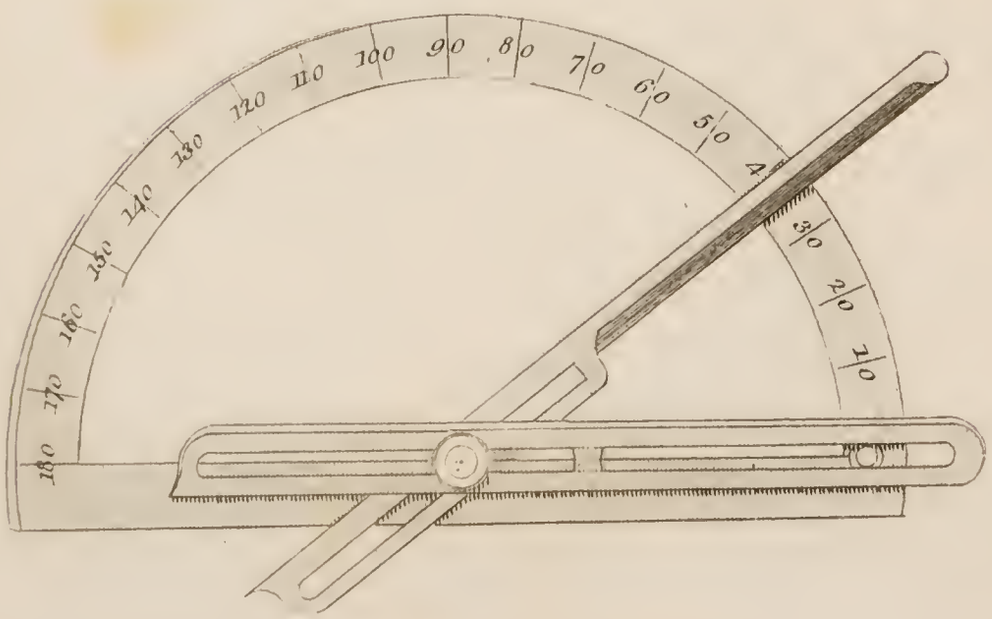

Mutlow Sc. Rugwell Gt.

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